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Contents

Sociology and Common Sense	CARL C. TAYLOR	1
Discussion	ROBERT REDFIELD	9
Discussion	SAMUEL A. STOUFFER	11
A Conceptual Scheme for the Study of Social Organization	GEORGE C. HOMANS	13
Internal Migration in Peace and War	HENRY S. SHRYOCK, JR. and HOPE TISDALE ELDRIDGE	27
Effect of the War on the Sex Ratio at Birth	R. J. MYERS	40
A Check on Underenumeration in the 1940 Census	DANIEL O. PRICE	44
Economic Differences Among Rural Centers	VINCENT HEATH WHITNEY	50
Intensity and a Zero Point for Attitude Analysis	LOUIS GUTTMAN and EDWARD A. SUCHMAN	57
Heuristic and Empirical Typologies: A Job for Factor Analysis	ROBERT F. WINCH	68
Alternative Meanings of Adjustment	WALTER H. EATON	75
Is Conformity a General or a Specific Behavior Trait?	ROBERT A. HARPER	81
The Sociology of Right and Wrong	THOMAS HANCOCK GRAFTON	86
Who Is the Criminal?	PAUL W. TAPPAN	96
Measuring Degrees of Verification in Sociological Writings... ..	HORNELL HART	103
The Sociology Curriculum and Teacher Training	JUDSON T. LANDIS	113

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Official Reports and Proceedings

Announcement of Election of New Secretary-Treasurer and Managing Editor 117

Announcement of Nominating Committee for 1947 117

Current Items

News and Announcements 118

Sebald Rudolph Steinmetz, 1862-1946 119

BOOK REVIEWS

Gottschalk, Kluckhohn and Angell: *The Use of Personal Documents in History, Anthropology and Sociology*. Florian Znaniecki 120

Hewitt and Jenkins: *Fundamental Patterns of Maladjustment—The Dynamics of Their Origin*. James S. Plant 121

Moore: *Industrial Relations and the Social Order*. Jerome Davis 122

Mayo: *The Human Problems of an Industrial Civilization*. Wilbert E. Moore 123

Mayo: *The Social Problems of an Industrial Civilization*. Wilbert E. Moore 123

Whyte: *Industry and Society*. Wilbert E. Moore 123

Glueck: *The Nuremberg Trial and Aggressive War*. John Dawson 124

Somerville: *Soviet Philosophy: A Study of Theory and Practice*. Joseph A. Leighton 126

Lorimer: *The Population of the Soviet Union: History and Prospects*. Warren S. Thompson 127

Schechtman: *European Population Transfers*. Amos H. Hawley 128

Economic Stability in the Post-War World—The Conditions of Prosperity after the Transition from War to Peace. Philip M. Hauser 129

Hutt: *Plan for Reconstruction: A Plan for Victory in War and Peace*. Rupert P. Vance 130

BOOK NOTES 130

OTHER PUBLICATIONS RECEIVED 135

American SOCIOLOGICAL Review

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1947

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SOCIOLOGY AND COMMON SENSE*

CARL C. TAYLOR

U. S. Department of Agriculture

THE THESIS of this paper is that if sociology is to develop into a useful discipline it must combine the type of knowledge and understanding which is derived by use of the most rigid technique of science and by the type of knowledge that is known among practical men as common sense. The combined knowledge derived from these two sources will not, and cannot be pure science, but it will not lack the validity of science in that it will be trustworthy in both its capacity to validate its findings and to predict social behavior.

By common sense I mean the knowledge possessed by those who live in the midst and are a part of the social situations and processes which sociologists seek to understand. The term thus used may be synonymous with folk knowledge, or it may be the knowledge possessed by engineers, by the practical politicians, by those who gather and publish news, or by others who handle or work with and must interpret and predict the behavior of persons and groups.

I shall offer no other defense of this broad use of the term than the fact that all the types of persons whom I have listed, and many others, use this term to describe their understanding of situations and processes

and contrast this type of knowledge with what they call "theory." A part of their knowledge may be due to their knowledge of science but a large part of it is not. Common sense is a body of knowledge possessed by groups of persons who generally have spent years, sometimes generations, in the processes of living, making a living, and planning for the future. It may in many cases be quite logical and even quantitative knowledge but not rigidly so as in the case of science.

Its genius is that it is largely qualitative and adaptive. In some cases scientists are able to arrange this same knowledge into logical constructs and even reduce it to quantitative symbols of expression and convert it into precise scientific generalizations. Such generalizations do not, however, completely convert common sense into science because the generalizations are almost certain to be so abstract as to leave out some of the subtle and useful understanding contained in the adaptive knowledge of common sense. This is due to no technical shortcoming on the part of the social scientist. It is due to the nature of the phenomena with which sociological generalizations deal and the necessity of abstraction in arriving at generalizations.

If large universes of social phenomena with which sociological generalizations must necessarily deal were always multiples of

* The Presidential Address read before the annual meeting of the American Sociological Society, Chicago, Illinois, December 28-30, 1946.

smaller universes of the same phenomena, then this would not be true, for then generalizations broad enough to constitute sociological laws could be constructed by adding together or pyramiding sound generalizations concerning lower or less comprehensive levels of human experience. Unfortunately, and by necessity, this is not the nature of either the larger or smaller universes of day-by-day human experience. The large universes while not entirely unrelated to the thousands of smaller universes which function within their time and space scopes are quite *different integrations* of the same persons and events which make up the thousands of smaller universes. Generalizations about them cannot therefore be the mere sums or products of sound generalizations concerning the numerous smaller universes. Each broader and higher level of generalization must leave out some meaningful aspects or characteristics of the smaller universes because it must confine itself to the common denominators of all of them and with components or elements of behavior not present in the smaller universes. A broad generalization about common denominators may be valid but it is so thin in its interpretation of the many aspects of human experience which are significant in smaller universes as to be largely useless in the field of practical social action. It may even be stated in quantitative conclusions which are statistically verifiable but such conclusions seldom cover all of the attributes of important social relations of day-by-day behavior.

I do not contend that it is possible to develop less abstract sociological generalizations or that abstract systems of sociological thought are useless. Sociologists will not, in fact cannot, perform their share of the common task of constructing a sound sociology out of a combination of science and common sense unless they continuously and fruitfully work in the field of social theory. I do contend that a sociology applicable to social action will not be constructed unless theories are passed down, step by step, through the various levels of human behavior about which they generalize, and are synthesized on each level with the common sense knowl-

edge possessed by men who must and do understand many things which cannot be expressed in either statistics or abstractions. These men of common sense, are men of practical affairs who operate on every level of human behavior and in every universe of social action which sociologists seek to analyze. Syntheses of what they know and what science reveals are not compromises. They are *creative* in that they amplify so-called theoretical knowledge, validate theories if they are correct and modify them if they are incorrect. It would be difficult to envision the magnitude, progress, and usefulness of sociology if it would systematically and diligently follow this path of development.

I am not presenting a brief in behalf of "the cult of the practical" or delivering a tirade against those who are said to dwell in ivory towers. I am not even contending that sociologists should "be practical" in the common meaning of that term. They can't be "men of common sense" in all the fields of human behavior which they must analyze. There is no reason to believe that the average sociologist, had he spent his life in any one of the specific areas of behavior about which he generalizes, could not and would not make practical application of his sociological generalizations to that area of behavior and action, but there are not enough hours in a day, enough days in a year, or enough years in a lifetime for him to participate in all the specific areas of behavior to which sociological generalizations apply and at the same time develop and practice the patient scholarship required to develop, or even understand, the generalizations which constitute science. He can accomplish much by specialization but he will find it exceedingly difficult, if not impossible, to be both a "man of science" and a "man of common sense."

The data which constitute sociology also constitute the information which men responsible for social action require in their day by day operation. Some men of practical affairs are highly conscious of this fact. They believe there must be some known laws of human behavior which can be applied

to the groups and processes with which they work. They have begun to call on sociologists, cultural anthropologists and social psychologists for assistance but are often baffled by the lack of knowledge, or the types of knowledge, which these social scientists possess. They have long been using physical scientists and generally do not understand the differences between the nature of physical sciences and social sciences. They therefore define their problem as X, Y, or Z, and expect the social scientist to furnish prescription A, B, or C, for its solution. Finding that the social scientist cannot do this they become discouraged or critical. The social scientist, in such situations, may not become discouraged, but what is worse he becomes cynical about "practical men." The dilemma is not unsolvable. If social scientists only knew and if practical men were forewarned in advance that it would require both the scientific knowledge possessed by the social scientists, and the common sense operational knowledge, possessed by the practical man, and the joint efforts of both in the acquisition of additional knowledge, neither discouragement, criticism, nor cynicism would develop. Instead, laboratories for functional social research would be established and competent personnel for their operation be guaranteed. It would be understood from the beginning that no engineer's solutions would be found but that the practical man and the sociologist would together complete the analyses with findings which would be valuable to both operational practices and to social science.

Opportunities exist for the establishment of such laboratories in every local community where sociologists live, in the states and nations of which they are citizens, and in the "One World" of which they are all a part. In each of these universes of social action are the phenomena a knowledge of which constitutes the science of sociology. In them men of responsibility and action are making decisions and influencing human events daily. Because they are men of responsibility they must make decisions and because they are men of action they must move with dispatch. It should be possible

for them to count on social scientists to help them move with a higher degree of surety. They as men primarily of action and common sense do not know the processes and techniques by which scientific social knowledge is obtained and validated and few social scientists know either the complexities or subtleties of situations with which practical men deal. If the two would join their efforts the knowledge of men of practical affairs would greatly amplify the body of sociological knowledge and the sociologist would add precision and surety to the knowledge and judgment of practical men.

To merely relegate a consideration of the validity and usefulness of what I have said to circles of logical and philosophical discussion in the faith that all the knowledge of common sense can or at least should be reduced to science will accomplish little or nothing more than to add another topic to the type of futile discussion in which too many sociologists have too long engaged. Creative synthesis of both thinking and acting as joint enterprises of men of science and men of common sense will bear more fruit. Such enterprises will establish the laboratories for which sociologists plead. They will be laboratories primarily in the fields of social action and this will be most fortunate because now relatively too large a portion of sociology consists of a knowledge of structure and too little of it consists of a knowledge of action. Most of the meanings in life inhere in action and sociologists should learn how to study action per se. It is only in action that the motives and attitudes, that is, the dynamics of persons come near enough to the surface of behavior to be observed.

There are of course many instances in which sociologists have studied action, in the fields of criminology, social pathology, social movements of various kinds, and many others. Too often, however, they have allowed the fields of social action to be occupied by social workers and so-called "social engineers" who are likely to possess neither adequate theoretical training nor the cultivated objectivity essential to social analysis. A simple but pointed illustration of

the application of social research in the field of social action will serve to demonstrate the combined use of technical knowledge and common sense.

An action agency, staffed with a highly competent corps of technical experts, operates with social units which were presumably democratically constructed and composed of farmers. The farmers want to attain the same objectives as do the technical experts and the action agency. The problem posed by the action agency was, "why is it that two Soil Conservation Districts located in the same locality and type-of-farming area, staffed by equally competent technicians, and composed of people of the same nationality composition, behave so differently. In one district our program is an outstanding success, in the other we must admit a high degree of failure." They called on sociologists to assist them in analyzing both districts. The sociologist assigned to the Project did not have any technical knowledge in the field of soil conservation. He did not even possess the folk knowledge of farming in the area, but he uncovered the folk beliefs and the attitudes and values of the farmer participants and non-participants, identified their accepted and trusted folk leaders and not only identified but revealed to the technicians and to the action agency some of the factors which were causing the difference in the social behavior of the two districts. He did this by gathering up, so to speak, the common-sense knowledge and folk beliefs of the farmers, synthesizing them with his own sociological knowledge, discussing his observations constantly with the physical science technicians and administrators, and writing a report of his findings. He made an appreciated contribution to both the farmers and the action agency and his study is worthy of publication as a social research document. Had he not been willing to join hands with men of common sense—the farmers—he could not have made his contribution as a sociologist. He made some quantitative analyses but his chief contribution was that he tapped the common-sense knowledge of practical day-by-day farmers and uncovered attributes

of their behavior and thinking which when synthesized with the technical knowledge of scientists will be a contribution to the solution of the problems of both.

The conflict between scholars in sociology who are expert in the field of quantitative analysis and those who engage primarily in qualitative analysis is more foolish than and not unrelated to the lack of cooperation between men of science and men of common sense. They too need to join hands in research. It is not enough that each is willing to let the other live and make his maximum contribution independently. Each needs the other if anything approaching complete analysis is to be made of most social situations. Statistics provide not only methods of verification and validation but also techniques for sampling large time and space universes which it is impossible to encompass within personal observations. There can be no conflict between the findings revealed by the most rigid methods of quantification and the necessity for use of the more subtle and often more significant understanding which can be gained only by personal, qualitative observation. Where conflict arises it is generally due to the statistician's willingness to disregard or eliminate from consideration those components of situations which cannot be quantified or the insistence of the qualitative analyst that he possesses something approaching intuitive understanding.

In recent years sociologists have probably made greater recognized and accepted contributions through statistical analyses than in all other fields combined. Psychologists have made outstanding contributions by use of both statistical and psychiatric methods. Cultural anthropologists and sociologists are however being called upon for contributions which go beyond either statistical findings or psychological analyses. What these sociological scientists need to do is to join hands with each other and with observers who are practicing participants in the social situations which they seek to analyze. Data on the age, sex, and ethnic composition of population units gathered by Census schedules, attitudes and opinions gathered

by questionnaires or interviews, and psychoanalysis of troubled or troublesome individuals, reveal only a part of what needs to be known about social situations. Social relations and social situations necessitate *research by groups of social scientists*, without which probably no very significant social discovery may be expected.

The methods of analysis of neither the statistician nor cultural analyst need be sacrificed in the least by a combined use of the two in social research. The procedure for cooperation should be simply to depend on the results of quantitative research to reveal the various contours of behavior within the universe being analyzed and then depend upon qualitative observations to describe the influence of elements or attributes of behavior within the universe for which there are no quantitative symbols of identification. In many cases the qualitative analyst will also identify other elements of situations which could and should be quantitatively analyzed. This is the procedure being followed in a rural social research project now under way in which an attempt is being made to use both quantitative and qualitative methods and in the qualitative part of the work to make use of the common-sense knowledge of persons who are participants in the social situations or universes being analyzed. The project (or maybe it should be described as a group of projects) was undertaken for very practical purposes and with a determination that in its conduct there would be no sacrifice of sound research procedures. It is an attempt to analyze a number of significant social universes concurrently.

The practical purposes are based upon the conviction that an understanding of the psychological and cultural components of agricultural and rural life situations is needed in order that administrators of agricultural and rural life programs, leaders of rural people, and farm people themselves may successfully deal with the social, economic, and even technical and physical issues of living and making a living by farming. The significant universes of behavior selected for identification, description and analyses were

certain socio-geographic universes of American rural life, rural regions, and five types of rural organizations—neighborhoods, communities, institutions, trade areas, and service or action agencies; also certain time universes in the fields of population, farm labor, levels of living, and scopes of organizational behavior and changes. Insofar as possible the time universes are being studied in the areas of the socio-geographic universes. The methods consist of, first, the maximum use of quantitative techniques, and then the use of qualitative or descriptive techniques in all socio-geographic, or cluster universes. Considerable use is made of participant observers, or what I have called here "men of common sense." Sociologists, cultural anthropologists, social psychologists, economists, and statisticians helped plan the project or projects and are being used in the laboratories and field work.

Major type-of-farming regions were accepted as regional universes, because their delineation is the end research product of a number of years of careful work on the part of agricultural economists who have attempted to group areas within which there is a marked uniformity of production—economic behavior. To the information used in the delineation economists and physical scientists are constantly adding data which are useful in social analyses. Furthermore, it is recognized that the routines of work and leisure; the daily, seasonal, and annual rhythms of farm life; and many types of social behavior and attitudes are sharply conditioned by the similar manner, means, and methods of making a living practiced by farmers in given type-of-farming areas.

Within regions all counties were grouped into strata and a county chosen from each stratum as a unit in the regional sample. In their selection highly technical statistical sampling methods were used. Anyone who desires to examine and appraise this contribution of experts in the field of quantitative procedures will find them described in the *Journal of the American Statistical Association* for September 1945, in an article entitled, "Component Indexes as a Basis for Stratification in Sampling." The authors of

this article were assigned the task of selecting approximately 70 counties, using whatever sources of data were available, which would represent the rural life of seven generalized major type-of-farming regions. Census data on county units were not and are not available on many types of social behavior which are to be studied in the counties. But county data were available on the relative magnitude of such significant type-of-farming enterprises as cotton, corn, dairying, etc.; on family labor, hired labor, number of days spent in off-farm work, and tenancy; on mechanization, as measured by value of farm machinery; on size of farms, and gross farm production. Data were available on mean age of farm operators; per cent of the counties' populations that were rural farm; change in civilian population and migration of farm population; per cent of farm population that is foreign born and nonwhite; value of home consumed farm products; and an index of the rural-farm level of living.

By use of these types of quantitative data, 71 counties were selected, from 6 to 13 representing strata within each of the 7 major type-of-farming belts, and 12 representing residual areas not included in these belts. Thus local areas (counties) were designated within which qualitative and quantitative social studies can be made with some assurance that the combined findings can be used in describing and analyzing the rural life of both the major type-of-farming regions and of the Nation as a whole. Incidentally, the statistical analyses accomplished in the stratification of all of the 3,056 rural counties of the country, and in the selection of the 71 sample counties, not only can be used as foci for qualitative studies within the counties but constitute just as valuable a body of knowledge in and of themselves as if they had had no other object than to be a good piece of quantitative research.

Counties are the sample areas from which are to be compiled data for time series analyses of population, farm labor, level of living, and rural organization changes and trends. Such data may be assembled from either primary and secondary sources of data to

be found in the counties or by enumerative schedules and questionnaires on statistically carefully selected sample areas within the counties. Qualitative studies use a combination of structural analysis and case studies, commonly used by cultural anthropologists. Each type of rural organization listed above is studied as a socio-geographic universe and as a unit of social behavior. Each type of behavior reflected in the quantitative data used in selecting the county, and in stratifying the other counties which it was selected to represent, is related to other facts which only personal observation can reveal and interpret. In this process and from participant observers the intangible but more abiding things by which people habitually order their lives are identified, described and analyzed.

When the analyst enters the county he already has in his possession a valuable body of quantitative data and a clear knowledge of what stratum of the regional and national universes the county represents. His first observations are therefore focused. Working with this degree of advance direction, he quickly begins to feel the pulse and heart beat of the people in the types of behavior which have manifested themselves in the recorded data which he has. His study of their formal and informal organizational life leads him quickly and far beyond this mere qualitative check on quantitative data. He lives among the people for three or four months, participates in their social affairs, talks to their best informed members, studies all local sources of recorded information and above all trades ideas about his observations with his carefully selected participant observers.

Participant observers are selected to represent the strata of whatever social structure exists in the county. They are not interviewed by means of schedules or questionnaires and there is no attempt made to record their responses quantitatively. They represent all levels of life and are made partners of the analyst in his attempts to uncover attitudes and values by which the people live. They are used as persons of common sense with whose knowledge and viewpoints the

analyst can check his own impressions and conclusions. They are participants in a much more immediate and wholehearted way than the analyst can possibly be and can therefore tell him things he could otherwise never learn. By cooperation with him in a common task they become constantly more expert and therefore more useful observers.

Field studies already completed reveal the shortcomings of the data available for stratification and for selection of counties but they also place flesh, blood, and nervous system on the skeleton of statistical information which is available on rural life and organization in the counties. Every thing they reveal adds to and none of it subtracts from the quantitative information. Furthermore, as we stated above, the quantitative analyses revealed certain contours of behavior within the county which help to focus qualitative observations.

Most cultural anthropologists would contend that three or four months is not sufficient time within which to accomplish complete analysis of the rural organization and life of a county. My response is, first, that complete analysis is not accomplished, or expected, or possible, and second, that in order to accomplish analyses of the universes we have selected for study we had, for example, to choose between spending two man-years in studying one Corn Belt county, three man months in studying each of eight counties, or one man month in studying each of 24 counties. If we were only compiling fairly obvious statistical data we would study 24 or 48, or maybe 96 counties and thus have a statistically much more valid sample of the Corn Belt. If we were making only a cultural anthropological study we might study only one county, or a small part of it, but we would have no sample of anything. We are trying to analyze large, significant social universes and trying to demonstrate that both quantitative and qualitative methods, and common sense are needed as techniques for doing it.

The examples I have presented to illustrate the practical use of the methods I am describing by no means demonstrate everything that can and should be done. Some of

the most significant observations which should be made are of situations and processes which are not and cannot be so rigidly constructed into research projects. They are observations made by sociologists who are a part of or thoroughly familiar with action programs such as the social security program, employment services, farm programs, labor adjustments, reclamation projects, the Indian Service, juvenile delinquency and many others. The contribution which can be made to the development of sociology by the valuable observations of trained sociologists serving in such social action programs is partly due to their capacities to make objective observations in the midst of action and partly due to the fact that they become possessed of the type of knowledge which I am calling common sense. Not least among their possible contributions to the development of sociology is the fact that they can and do convert a considerable body of social science into common sense knowledge by making it part of the working knowledge of those responsible for practical programs of social action.

Any attempt to combine quantitative and qualitative methods of observation and analyses and the synthesizing of the knowledge of science and the knowledge of common sense in actual research practice will reveal the shortcomings of each method in ways which are not obvious when each is used separately. Statistical methods alone have no capacity to identify attributes of phenomena which are not included in their quantitative data. The statistician is helpless, no matter how aware he is of this weakness, because some attributes of social phenomena do not manifest themselves in such ways as to be susceptible of quantification. Qualitative observation identifies the presence and the influence of these attributes and guarantees that they not be disregarded. The qualitative analyst working alone is also faced with difficulties which he may not, and apparently quite often he does not, recognize. His laudable attempt to study the total situation, all components of it and all attributes of all phenomena within it, some-

times results in a magpie's nest full of descriptive information most of which is only slightly analyzed. The imperfections of his methods are largely due to failure to record observations with sufficient precision to make comparative analyses possible and to the fallacy that there is a possibility of completely describing a social situation. If his partner in research is a quantitative analyst he can be assured that his imperfections will be called to his attention.

The creative and practical synthesis of science and common sense and the joint use of quantitative and qualitative methods of research will automatically serve to sabotage the sociologists' futile and false practice of counseling with perfection. It may, of course, cause him to abandon some of the pleasures he enjoys in his semi-esoteric ways of life and it will deflate or at least modify his expectations of social discovery. This I believe will be salutary and ultimately contribute to the usefulness of sociology and its development as a sound discipline.

I have for a long time worried about the fact that it takes young sociologists from five to ten years to recover from what happens to them in their graduate training. Many of them seem to become so deeply imbued with the conviction that they are capable of developing new systems of sociology or of making some great social discovery that they are almost incapable of patient, painstaking analyses of living social phenomena. They should be taught that there is no such thing as individual social discovery; that sociology is the study of the behavior of persons in relation to each other and that everything they can ever know about this behavior and these relationships is already known in some of its aspects by those who are involved in them. They should be taught that they can at best only hope to furnish part of the answers of what these relationships are, how they function, and why. They should be taught that their possibility of being Einsteins in the field of sociology is an utter impossibility because of the modifiable, even creative, nature of human behavior and group relations. If they

could start their mature professional careers with this understanding they would know that they should seek to join hands with others in fields of research which require many hands and minds.

Social discovery is a product of the knowledge of persons living on many levels of experience. Intellectuals play important, but not the only, roles in such discovery. Men of common sense, quite often unconsciously, also have played leading roles in every phase of the evolution of social knowledge. Much would be gained if they would become as conscious and proud of the part they play in the social field as they are in their contributions to discovery in the fields of business and mechanics. That they are not is due to the fact that they do not know the process of social discovery. To them many of the things which social analysts seek to understand are perfectly obvious. They seldom, however, see the social significance or implications of the obvious and are baffled by the sociologists' concern with them. They are also often baffled by and confess that they don't understand why persons and groups with whom they work act the way they do. Few of them know that they could submit their problems to laboratory analysis by joining hands with sociologists. Much less do they know how much sociologists need laboratories in which they can study living human relations.

Such laboratories will not be brought into existence by writing social theory concocted out of "higher criticism" of the doctrines of dead or living social philosophers, however erudite such criticisms may be. They will not be brought into existence by the formulation of "systems of thought," useful as such formulations are. They will not even be created by public opinion and attitude polls, enumerative schedules, and the statistical analysis of their findings, notwithstanding the invaluable contributions these technologies and techniques have made to social analysis. They can be established by men of common sense, who operate or influence these laboratories as a means of attaining goals which are not per se scientific, joining hands with social scientists whose goals

are careful objective analysis for science's sake.

Because the laboratory of the sociologist is social action which is not stimulated or induced by himself but by others, and because he seldom can be a whole-time participant in the types of action he seeks to analyze and understand, there is nothing more defeating than for him to try to work alone. He needs not only to join efforts with men of common sense but to join hands with professional colleagues in *group research*. It would be fortunate if he could be a member of such face-to-face groups as have worked together for years on certain large sociogeographic universes at the University of Chicago and the University of North Carolina, or could be financed for specific research projects as were Warner and colleagues, the Lynds, and others in their group research projects. This however is not a necessary condition for group research. What is necessary is a greater degree of mutual understanding, purpose, and appreciation among sociologists, and a deeper conviction that science is constructed chiefly out of the findings of research. At the present time sociology consists chiefly of a body of social theory about some of which there is a great deal of disagreement. The disagreement would be positively healthy if it led numerous adherents of the same schools of thought systematically to join hands and sustained efforts in the use of its tenets as hypotheses for actual research. Its various adherents could be located at many places in the world and still work as a group. In some fields of research this would be a distinct advantage, in fact greatly to be desired. In other cases it would be neither desirable nor necessary. It could be, but all too seldom is, done by a group of sociological scientists located in the same institution. My guess is that it would be done more frequently if more of them believed that some of their best laboratories are, so to speak, on the streets and farms that lie in their own back yards; and done still more frequently if they believed that some of their best colleagues in social discovery are the men of common sense who live by the relationships

and processes which sociologists seek to analyze, understand, and explain.

This paper is a thesis and a preachment. The thesis is that good sociology is a combination of science and common sense and the preachment is that few sociologists have both these types of knowledge. The experiences which lead me to these conclusions are the fields of social action which I have observed during my professional life, in some of which I have participated fairly intimately. I am anxious to broaden the base of these types of observations and to submit my conclusions concerning them to criticism and discussion. I have therefore invited two other sociologists who have also participated in social action programs to join me in this discussion.

DISCUSSION

by

Robert Redfield
University of Chicago

I agree so heartily with what Carl Taylor has just said that the remarks I am about to add are not likely to be helpful. Applause does not clarify. I agree that the usefulness of sociology is increased as commonsense knowledge of social situations is taken into account by sociologists. I agree that the more inclusive generalizations of sociology do not often serve as guides to practical action. I agree that a combination of statistical and non-statistical descriptions of many or most social situations is more illuminating than is one of these descriptions alone, and that when a quantitative sociologist works together with what Carl Taylor calls a qualitative analyst, each is likely to supplement and correct the other. And I see as much to be gained from group research as he does.

In searching for something to say that would be more than an approving echo, I find I could work up a little dissatisfaction with the way he has made the distinction between commonsense knowledge and qualitative analysis. According to Dr. Taylor, there are three principal parties to a well-conducted study of a social situation. (I will here leave out the theorist who may have provided some influential systematic ideas and attend to the three collaborators emphasized in Carl Taylor's presentation.) Two of the three come from universities. One is the

statistical sociologist. The second, the qualitative analyst, is, I suppose, myself and hundreds like me, cultural anthropologists and sociologists who study social situations by direct observation, "in the field." The third, for whose participation Dr. Taylor chiefly argues, is the man of common sense. He is a fellow who lives in a community and as a part of his business of living tries to get something done working with the people of that community. He may be a leader of a farmers' organization. He may be a precinct committeeman. What is important about him, Dr. Taylor reminds us, is that he has an understanding of how things work or how they don't work in that community.

Now we might get an impression from Carl Taylor's address which I don't believe he wants us to get. This mistaken impression would be that it is *only* the man of common sense who is to provide the general view of the situation, the gleam of general understanding, the clue of explanation. I don't think he wants us to understand that the man of common sense supplies the explanations while the qualitative analyst collects the facts and the statistician counts them. Rather, I should say, it is the obligation of the cultural anthropologist or field sociologist to "size up" the whole situation and to provide some general statement of what the situation is like and where it is going. This is what we mean by "insight," and I should say that the cultivation of insight is a principal business of the qualitative analyst. In this regard there is a great similarity between him and the man of common sense. The sociologist or anthropologist has to stay long enough and intimately enough with the situation to provide this insight, this general way of taking the situation as one piece and throwing emphasis on some parts of it as particularly meaningful.

Moreover, I would not join in the view, if it were Carl Taylor's view, that the understanding of the man of common sense is always better than that of the sociologist coming into the situation from outside. Was it not Ellsworth Faris who used to say that common sense is always wrong? Always wrong it is in that it is never complete, and is never understanding in terms of theoretical and comprehensive generalizations. Often wrong it is, insofar as it rests on folk beliefs and prejudices. In this respect there is an important difference between the qualitative analyst and the man of common sense. Both use insight. Both get a comprehensive understanding of the total situation from direct ac-

quaintance with it. But the view of the man of common sense suffers from the disadvantages as well as the advantages of three circumstances from whose evil effects, or beneficial effects the sociologist from the outside is free: he is only inside the situation and cannot see it from outside; he does not know many comparable situations; and he is not trained in methods of correcting his own view. So the sociologist, while he surely will be wise in listening to the man of common sense, must be prepared to find him wrong as much as he is right. It was common-sense workmen in the Western Electric factory who told the sociological investigators that the foremen were to blame for the difficulties of production, and we all remember, who have read the sociologists' reports, how little the foremen had to do with the real explanations.

We will agree that a fully useful sociology is not to be made by systematizing what one sociologist happens to know. We will urge the necessity of direct observation of particular social situations, and the combination of statistical with quantitative methods. We will recognize insight as indispensable to the understanding of social situations and therefore to social science. We will see in action programs an excellent opportunity to multiply insights. We will recognize, also, that all life is, in a sense, an action program, and that the anthropologist who studies a primitive community finds its leaders and its factions engaged in efforts to get *this* accomplished or *that* prevented, even without the stimulus of the United States Department of Agriculture. The point at which we should look, it seems to me, in connection with Carl Taylor's address, is the point he calls "synthesizing common-sense understanding with sociological knowledge." How may the sociologist best take advantage of the insights of the men of common sense within the community that is studied? To what extent is it helpful, to what extent harmful, for the sociologist to be *himself* a participant in the action program? What are the ways of rapid access to the common knowledge? And how are the insights of the men on the ground best to be tested and corrected? Few are likely to doubt that the sociologists in Dr. Taylor's example of the study of success and failure of an action program in two soil conservation districts were wise in talking to the farmers in those districts. How else could one find out what were the significant differences between the two districts? But just how, one would ask Dr. Taylor, was the common-sense understanding of the

farmers "synthesized" with Dr. Taylor's more formal knowledge? How, in the first place, was it determined that what the farmers said was true or not true? And in the second place, how was this knowledge, in the language of common sense, converted into the more formal language of science? For the making of social science is a going back and forth between particular fact and general fact, between the partial insights of men on the ground and the improved insights of the more theoretical men who come freshly to that ground. We need accounts of how this is done in particular cases to quicken our scientific imaginations. In reminding us of this need, and of a place to look for its fulfillment in the jointure of man of action and man of science, Carl Taylor has done us a service.

DISCUSSION

by

Samuel A. Stouffer
Harvard University

Our Society is honored in having as its President a man whose career has been a living exemplification of the thesis which has been so effectively discussed this evening.

I believe sincerely that the rapidity with which sociology will develop as a science depends—just as was the case with chemistry, physics or biology—on the usefulness of its engineering applications. Science in general owes its support primarily to the fact that as a result of scientific study *something happens*. If the engineering applications of sociology in industry and in government really pay off, the salaries of and prestige of men with sociological training will increase, and this in turn will attract more and better graduate students. Thus the level of our professional competence will go up. At the same time the engineering needs become a powerful stimulus to the development of better social theories—theories which are operationally stated in such form that inferences and predictions from the theory can be empirically tested and, if good, applied in practical situations. Nothing could be more wholesome than pressure on the theorist to abandon the slippery verbalisms which give him an out even when he is wrong and to force him to put his propositions in such specific terms that if he is wrong the data will show that he is wrong.

The trouble with social engineering as an application of sociological theories today is prob-

ably the fact that the theories are not too helpful. They have not often been put to critical test because they have not been formulated so that they could be proved wrong if they are wrong. Hence the social actionist darned well better have *common* sense, since he cannot get much real help from *uncommon* sense. I suspect that Dr. Taylor has overstated rather than understated in giving sociological training as much credit as he does for making a man useful in a social action program. He does say, of course, that it takes a young man several years to unlearn what he had been taught in graduate school. But, I am inclined to think that neither Dr. Taylor himself, nor those of us who have played humbler roles in social action programs, owe much of what success has been achieved as social actionists to sociological theories as such. Those who are successful owe it to the fact that they have a good endowment of practical horse sense, plus technical skill in ordering data. Sociology is not a mere collection of statistical facts or of items of practical experience or even of tricks for studying these things. Essentially sociology is a system of theoretical propositions about cultures, societies, and groups. And I fear, indeed, that when we speak of the engineering application of sociological theories we may be talking largely of the future, rather than the past.

Let's check up on this apprehension. Dr. Taylor gives some interesting illustrations of the sociologist in action in studying rural problems. But, you will note, not once does he mention an explicit sociological hypothesis or theory which was used in these studies. Now I do not mean broad statements like "Values are relative," or "There are many causes of social change." I mean propositions of the type, "Given A and B under condition C, the result will tend to be D." Where can we find such propositions, which can be usefully applied in a social action program?

I am sure, of course, that there must be some examples to guide us. Dr. Taylor doubtless has examples which he lacked time to present in detail. One illustration which comes to my mind is from the work of Shaw and his colleagues on juvenile delinquency. Years of research led to the conclusion that much of the urban delinquency was concentrated in a few areas, characterized by poverty, bad housing, and apathy on the part of most of the elders who, being unsuccessful economically, had been unable to join the more successful in migrating

to better parts of the city. The more enterprising of the elders among these ecological dregs remaining behind tended to be associated, directly or indirectly, with racketeering and graft, and served as role models for the youth, especially if glamorized by a prison record. On the assumption that these youths were not "psychopathic personalities"—that it was not abnormal but normal for youth to emulate the value system of the immediate role models—it would follow that if the value system of the role models could be altered, the value system of the youth would change too. So Shaw and his associates went to work with the adults of a community to help them develop a new *esprit de corps* and sense of identification with the value system of a larger society. If the sociological theories are correct, a change in the value system of the adult role models should reduce juvenile delinquency. Here is an example of the explicit application of explicit theory which can be stated operationally. How many examples like this can we find? If there are a good many, why doesn't somebody write a book about them?

Now why haven't we more theories, capable of explicit operational statement which can be of service to the social actionist? Somehow, I do not quite think that Dr. Taylor's glorification of common sense will help us much in answering that question. Indeed, I would almost say that what we need is more *uncommon* sense. Common sense led to considerable progress in horse breeding, but the modern theories of heredity are a far cry indeed from common sense. The concepts which have shaken social psychology out of its lethargy are not common sense. What about Freud? Indeed, the proposition might be advanced that until we are willing to dare, like Mendel in heredity, like Freud in social psychology, to think in uncommon sense patterns, we are not likely to improve on the polysyllabic and obscure statements of the obvious which bulk so large in sociological thinking.

Along with this daring to throw common sense overboard, must go an effort to put the ideas to crucial test. And here, if we don't watch out, social action programs will get us into trouble. We may not have the patience to test

our ideas in small model situations which give us enough control of the relevant variables to enable others to see that our theory is wrong if it is wrong. Please remember that though the wheat crop of Europe was important and needed action to improve it, Mendel studied *wrinkled peas*. It may be ultimately more important for a Lewin or Marenco to work out some theories of group dynamics on groups of four or five persons, with careful experimental controls, than for a sociologist to study the race prejudice of a million people. The pressures on us—the cry of an anguished world for help in the solution of our great social problems—must not be allowed to force all of us into taking the short view. Some must take the long view. Daring to use *uncommon* sense, able to formulate our thinking so that if it is wrong we can be proved to be wrong, and willing to design scientifically controlled experiments which those in a hurry will call trivial, we must work with a timeless patience in forging sociological theories which eventually can be applied by social actionists for the betterment of mankind. Thus social engineering—as is so seldom the case today—will derive *directly* from social theory. Then, as social engineering pays off in richer and richer dividends, we will attract to sociology an increasing share of the best brains of our college youth.

Mr. President, we are indebted to you for your formulation of the problem under discussion tonight. Your emphasis on the importance to sociology of social action is in healthy contrast to the remark of one of our great sociologists who, when a student affronted his olympian detachment by asking him what could be done to alter a bad social situation he had been analyzing, proudly replied, "Not a thing, young man, not a God damn thing."

Sociology can do something, will do something, by putting into the hands of social engineers not just statistical facts, not just unordered items of experience, not just vague generalizations, but tested theoretical tools which will work in practical applications. That is our faith. In that faith, we are proud to be sociologists.

A CONCEPTUAL SCHEME FOR THE STUDY OF SOCIAL ORGANIZATION*

GEORGE C. HOMANS
Harvard University

IT is the office of theoretical investigation," said Willard Gibbs, "to give the form in which the results of experiment may be expressed."¹ If he had put "observation" for "experiment," Gibbs would have stated the purpose of the present paper. It is to provide one form in which may be expressed what we know about social organization. Please note, at the beginning, that more than one form is possible and that, in a limited space, the one suggested here can only be sketched out.

In the field of social organization we now have a great deal of fact. We have descriptions of social groups of many different kinds, from primitive societies to modern factories and communities. Granting that these studies are not everything they may sometime become, many of us feel that they are closely observed and clearly described. We feel also that social groups, no matter how far separated in time or purpose, have certain things in common just because they are examples of organized human effort. We feel that in the necessities of organization itself and not in factors which are often treated as independent of organization: physical environment, racial heredity, the market, infantile experiences, or culture tone, must, in at least one instance, be sought the reasons for the differences and similarities between societies and for changes within societies. Here there are no primitive, industrial, religious, rural, or community sociologies; there is only one sociology—a sociology of organization. For an analytical science, any group is a microcosm.

We have a great deal of fact; we also have a great deal of theory. Some of it has been useful in describing particular kinds of

social groups: it needs to be stated with full generality to apply to all groups. Some of it has been stated half-intuitively: it needs to be "spelled out." Some of it has been beautifully clear and explicit, but partial: other elements of theory need to be added to make a satisfactory whole. Yet, whatever the limits of particular statements, there have been signs of convergence, and the present paper tries to present a conceptual scheme toward which some of our theories may be converging. The paper only brings out what has been latent. It puts together things which have been lying around for some time in the literature. It is eclectic rather than original. It takes what it needs where it finds it.

There are signs of convergence. It might be faster if we learned from the experience of the older sciences. Here, stated crudely, are a few of the rules which this experience has shown to be necessary in setting up a conceptual scheme. They have to some extent, perhaps not sufficiently, guided the building of the present one:

1. Look first at the obvious in its full generality. Only then does science economize thought.
2. Do not use high-order abstractions until you have exhausted the possibilities of low-order ones.
3. Talk about one thing at a time. That is, in choosing your words see that they refer not to several classes of fact at the same time but to one and one only. Corollary: once you have chosen your words, always use the same words when referring to the same things.
4. Once you have started to talk, do not stop until you have finished. That is, describe systematically the relationships between the facts designated by your words.

* Manuscript received September 23, 1946.

¹ M. Ruykeyser, *Willard Gibbs* (1942), p. 232.

5. Science consists of the "careful and complete description of the mere facts."² It drops the "why" and looks at the "how."
6. Cut down as far as you dare the number of factors considered.
7. Recognize that your description must be abstract, since it deals with only a few elements of the concrete thing. Recognize the dangers of abstraction, especially when action is required, but do not be afraid of abstraction.

Perhaps the reason so few of us carry these rules into effect is that they could not be better calculated to make our books and articles dull reading. We still work in the literary tradition, however badly we live up to it, and the rules of writing contradict the rules of theory-building at every point. In writing, the obvious, or what looks like it, is the thing you are most careful to avoid. Since it hurts to talk about one thing at a time, you use words which refer to several things at once. You also use different words for the same thing. If you do not, you lack variety. Systematic discussion, too, is notoriously repetitious, because the same things must be considered in several different connections. Finally writing is always concerned, and must be concerned, with giving a vivid impression of the concrete reality, and its success in doing so is the measure of its charm. The exposition of a conceptual scheme makes hard reading because it breaks all the rules of good literature, but only by breaking these rules and sticking to others will it become science.

The elements of social behavior. The present paper presupposes the direct observation of social behavior. It asks the devastating questions: Looking at the actions of men with eyes innocent of the usual preconceptions what do we see? What simple classification can we start from in this field of fact? Attempting to answer, it sets up, as components of the conceptual scheme, individuals and three elements or determinants of the behavior of individuals in

groups, which will be called *operation*, *sentiment*, and *interaction*.³

In ordinary language, *operations* mean the things that men do: operations on the physical environment or on other human beings. The full range of actions included here should be noted. Eating, drinking, plowing a field, tending a machine, putting on a coat, dancing, performing a ritual, and of course talking, though talking gives rise to special problems—all these are labelled operations. What they have in common appears to be some use of the muscles of men. No word is more than a ticket, but the use of the word *operation* here has some drawbacks. It must not be confused with Bridgman's operational theory of concepts, and was chosen because other suitable words had already been taken up. *Work* has a special meaning in the conceptual schemes of physics and may sometimes have an analogous one in sociology. *Behavior* and *action* are perhaps better applied to the whole of which operations are a part.

The definition of *sentiment* is more difficult. If we consider what we mean by the word, all that sentiments have in common seems to be some connection with internal states of the body, not well described except for the grosser sentiments: Cannon's pain, hunger, fear, and rage. In sociology we do not observe sentiments but operations which we take to be manifestations of sentiments, in facial expression, in bodily attitudes, above all, in what people say. Upon the whole, though, throughout human experience, men have successfully acted on the assumption that they could infer the existence of sentiments from what they could see and hear people do and say, and this assumption will have to satisfy us here. Sentiments are a concession to common sense. Note again the full range of things called sentiments here: from fear, hunger, thirst, and lust to such far more complicated matters as liking and disliking for individuals, approval and disapproval of the things they do. The psy-

² E. Mach, *The Science of Mechanics* (1942 ed.), p. 190.

³ For a preliminary statement see G. C. Homans, *English Villagers of the 13th Century* (1941), pp. 405 ff.

chologists do not use the word so broadly. The present use applies only to the present circumstances.

The behavior of a man living by himself would exhibit both sentiment and operation. What is it that makes behavior social? This element will be called *interaction*. When we refer to the fact that the operation of one man is followed, or, if you wish, stimulated by the operation of another, and so on in chains, entirely in abstraction from the particular operations performed or sentiments manifested, then we are referring to interaction. It may be especially difficult to think of interaction, consistently, as separate from the other elements of behavior, but it seems to be necessary to keep it separate and in much of our thinking we do in fact keep it separate without admitting as much.

Each of the elements named here has been used as a concept by social scientists. For instance, Roethlisberger and Dickson are discussing the Bank Wiring group at the Hawthorne Works of the Western Electric Company. They point out that the group held certain sentiments about such things as the restriction of output and go on to say: "It may be concluded that the individual's position in the group was in large part determined by the extent to which his behavior was in accord with these sentiments."⁴ If you will examine what the authors mean by *position*, you may agree that they mean, in part at least, habitual position in the chains of interaction among the members of the group. By *behavior* they refer to the element of operation, and the word *sentiment* with its meaning are the same for them and the present paper. Further, they are describing a state of mutual dependence between the elements of behavior. One element does not vary in independence of the others.

Again, Roethlisberger and Dickson are describing the methods used in analyzing the behavior of the Bank Wiremen: "Each occurrence in which a person entered into association with another person was examined to see whether the relation thus manifested

expressed an antagonism, a friendship, or was merely neutral." Here they are talking of the sentiments. They speak further of what they call *participation*: "Two questions were asked: (1) To whom do this person's relations extend? Does he associate with everyone in the group, or are his social activities restricted to a few? (2) Does he enter a great deal or relatively little into social relations with the people with whom he associates?"⁵ Here the authors are looking at the extent and frequency of interaction.

By far the best discussion of interaction as a determinant of behavior is that of Eliot Chapple in a brilliant paper which is too little known.⁶ The present definition has been taken from him. To him a more general idea can also be traced. It is not enough to discriminate once between the elements of behavior. It is still more important to keep them discriminated and never let the old confusion return. Unless they are kept distinct it is impossible to consider the relations of mutual dependence between them. The only criticism of Chapple which will be made here is that he stopped too soon and did not apply to the elements of operation and sentiment the kind of analysis that he applied to interaction.

Operation, sentiment, and interaction have been called elements rather than variables. For quantitative observation and mathematical treatment, specific variables must represent the elements. It is possible to measure the quantity of the operations of a group (through output records), the extent, frequency, and order of interaction,⁷ perhaps even the strength of sentiment. Breaking the elements down in this way is one of the next steps to be taken in studying some kinds of social behavior. But a warning is needed. Sociology will miss a great deal if it tries to

⁴ *Ibid.*, p. 493.

⁵ E. D. Chapple, with the collaboration of C. M. Arensberg, "Measuring Human Relations," in *Genetic Psychology Monographs*, Vol. 22 (1940), pp. 3-147. For the definition of interaction see p. 24. See also E. D. Chapple and C. F. Coon, *Principles of Anthropology* (1942), pp. 36-41.

⁷ *Ibid.*, p. 25.

⁴ F. J. Roethlisberger and W. J. Dickson, *Management and the Worker* (1939), p. 552.

be too quantitative too soon. Data is not nobler because it is quantitative nor thinking more logical because it is mathematical. The old-fashioned naturalist, who only used his eyes, was also a scientist. The last emphasis is always on the immediate situation. Nothing which can illuminate it must be ruled out for doctrinal reasons, methodological or political. We shall be blind enough without wilfully narrowing our vision. At the same time, we do not have to learn the hard way. The older sciences have already struggled with the same general problems as sociology. If the solutions have been stated mathematically, they are not to be disregarded just for that reason. No matter where it comes from, we shall need all the help we can get.

There must be a number of possible classifications of the elements of social behavior. The one made here is crude but will be crudely used. It is designed to develop some of the more obvious generalizations of sociology and can be judged only by its usefulness for that purpose. Other men have used such concepts as *status*.⁸ There can be no objection, provided the concepts are definitely related to observed behavior. But *status*, to stick to the example, refers, as commonly employed, to a complicated combination of the elements of behavior. Might it not be wise to establish the first-order abstractions before going on to the second-order ones?

One word about individuals, who are components of the conceptual scheme. It may seem too great an abstraction, but one of the assumptions which must be stated candidly is that physical and mental differences between individuals do not come into the scheme. The fact that a person is a male or a Mongol or an idiot is less important than the fact that he takes a certain part in co-operative activity. In the same way, the fact that he is a father or a president or a pope is less important than his position of leadership. For the purposes of the present conceptual scheme, a proviso which must al-

ways be understood, the differentiation made by organization is the only directly significant one.

The primary and secondary systems. In the present scheme, the elements of social behavior are described as mutually dependent in two systems, which will be called the *primary* and *secondary* systems. As usual, these words do not imply that the primary system is earlier or more important than the secondary. They are mere tickets, indicating only that it is sometimes easier to begin the discussion at the primary system. The words, with the insight behind them, come from W. L. Warner. For instance, he writes: "The economic life of a people is essentially concerned with relating the primary technological adaptation to nature and the community's secondary adaptation which is its social organization. . . . The tools and implements are formed into a general order of making and using to exact a supply of food and other creature necessities from nature, and they are then used by the population of a group in a systematized manner through a set of conventions and social usages which are dictated by the social organization. The social organization regulates the technology and helps discipline the distribution and consumption of its productivity."⁹ This comes from Warner's description of an Australian black-fellow group, but he applies the same discrimination to a modern American community. In the first volume of the *Yankee City* series, Warner and Lunt write: "The type of behavior by which a group adjusts itself to, and partially controls, the natural environment is, as we have said, its technical system; the system of adjustments and controls of the interactions of individuals with each other is the social organization. . . ."¹⁰ Here Warner calls the primary system the technical and the secondary system the social, but the more neutrally-

⁸ W. L. Warner, *A Black Civilization* (1937), p. 138, also p. 10. A similar classification, in which the line between the two systems is not drawn as it is here, is found in R. LaPiere, *An Introduction to Sociology* (1946), p. 162-3.

¹⁰ W. L. Warner and P. S. Lunt, *The Social Life of a Modern Community* (1941), p. 21.

⁹ R. Linton, *The Study of Man* (1936), pp. 113-131.

colored terms may be preferable. The distinction appears to be the one Roethlisberger and Dickson make between "forma" and "informal" organization in a factory.¹¹ Their language is well adapted to industry but is misleading for the study of social organization in its full generality, since in many societies the thing which Roethlisberger and Dickson call "informal" is highly formalized.

In trying to bring out what is latent in Warner's words, this paper makes a further distinction. Put on one hand the environment on which a social group operates, and the plant and tools (including farm animals) with which it operates. Put on the other hand the organized human behavior which makes up the primary and secondary systems. All three items: environment, plant and tools, and systems of human behavior are important and mutually dependent in the concrete phenomena. Here, however, no systematic attention will be given to the first two. They will be taken as given in any particular instance. The picture is of an isolated system whose exchanges with its environment are known. This distinction is like the one Barnard makes between a "co-operative system" and an "organization," the latter being defined as a "system of consciously coördinated personal activities or forces."¹² Note that the environment is not always the natural environment of the biologist. It is always relative to the group considered, and may include human beings not members of the group. A law court is operating on an environment as surely as a primitive tribe.

Warner discusses social organization in terms of three systems not two. To the technical and social he adds the ideological, the last consisting of the intellectual schemes, the "absolute logics" by which men interpret their world to themselves.¹³ The ideological

system will not be considered here. In part it is determined by and in part it determines the form of the technical and social systems, but it is different in kind. What we observe of it consists wholly of what men say, write, or depict. Like the environment and the tools, it is of the greatest importance in the concrete phenomena and is left out of systematic consideration only to make the problem more manageable.

The description of the primary and secondary systems must begin somewhere, and it begins here with a group of individuals. A group is defined by interaction. The individuals A, B, C, D, E . . . are members, as we say, of group I. Within a given period, A interacts more frequently with B, C, D, E . . . than he does with M, N, L, O, P . . . whom we choose to consider outsiders or members of group II. B also interacts more frequently with A, C, D, E . . . than he does with any one of the outsiders.¹⁴ And so on. It may be true that E interacts about equally with some members of group I and some of group II and so forms a link between groups. In any event, it is possible, merely by counting the interactions, to map out two quantitatively distinct groups. But note again that this definition of a group is entirely relative. It depends on the group you choose to consider. The United States of America is a group in the sense defined, a group of the sort we usually call a society. A society is divided into a complicated hierarchy of sub-groups, and any group with a population larger than two can be divided in this way into sub-groups.

The primary system. The primary system will be considered first and then the secondary. In the primary system, the elements of social behavior are represented as follows. The operations are the ones the group performs on its environment, with the tools at its command, as a result of its initial sentiments. For a primitive group, these are the operations of hunting, fishing, gathering, and the like, the punishment of crime, the

¹¹ Roethlisberger and Dickson, *op. cit.*, pp. 558-62 and elsewhere.

¹² C. I. Barnard, *The Functions of the Executive* (1938), p. 72.

¹³ W. L. Warner and P. S. Lunt, *The Social Life of a Modern Community* (1941), p. 22; W. L. Warner, *A Black Civilization* (1937), p. 11.

¹⁴ G. C. Homans, *English Villagers of the 13th Century*, p. 403. For a demonstration of the method see A. Davis, B. B. Gardner, and M. R. Gardner, *Deep South* (1941), pp. 147-51.

education of the children, and, in the fields where useful operations on the environment are impossible or inconceivable, the performance of rituals. It is useless to go into the whole list. For a group which is not in immediate contact with the natural environment, but is a sub-group of a larger group, the operations performed are those set for it by the larger group. Thus a body of men may be casting engine-blocks in an automobile factory or handing down legal decisions in a court house. In any case, the operations the group performs in the primary system are those it carries out on whatever constitutes its particular environment.

✓ Sentiments are represented in the primary system by the ones which men bring to a group as distinguished from the ones induced in men by the action of the group upon them. For a primitive group, or any other group in immediate contact with its natural environment, these are the sentiments of fear, hunger, thirst, and any other so-called primary drives that we wish to mention. For a working group in a factory, the sentiments in the primary system are much more complex: need for something to do, need to support a family, need for association with others. Such sentiments cannot be called primary in themselves, since they are induced in men by the action of social groups, but they are primary with respect to the group considered. If the sentiments are the ones a member brings to the group, the analysis remains the same whatever their origin.

➤ Finally, the pattern of interaction in the primary system is the one necessary to put into effect the operations required. Here two kinds of interaction can be distinguished, which Barnard calls *lateral* and *scalar*.¹⁸ For a factory group, lateral interaction is illustrated by the man who paints an automobile mudguard and then sends it on to another man who puts it into the final assembly, scalar interaction by the foreman who, to use the conventional phrase, coordinates the work of several men. The or-

ganization chart shows the intended pattern of scalar organization in the factory.

Mutual dependence of the elements of social behavior. In neither the primary nor the secondary system are the elements of social behavior independent. They are in a state of mutual dependence with the environment, plant and tools, ideologies, and with one another. Only the last relationship will be considered systematically here: the mutual dependence of operation, sentiment, and interaction.

The three determinants of social behavior are mutually dependent. Only by mathematics can such a situation be described adequately, and mathematics for the moment we cannot use. We do not have the indices which could turn the determinants into variables. Instead we are forced to use ordinary language, which is equipped for handling only one independent factor and one dependent factor at a time. Here, therefore, the mutual dependence of the determinants will be considered by pairs, of which there must necessarily be three: sentiment-operation, operation-interaction, and interaction-sentiment. This method seems incapable, but its difficulties must be faced. For instance, it is easy to say that the determinants interaction and sentiment are mutually dependent, but when we go on to say just how they are, we are forced to assume something about "other things being equal." L. J. Henderson was fond of saying: "People talk about 'other things being equal' without saying at what point they are equal." In discussing the mutual dependence of interaction and sentiment in the secondary system, we may say: "Other things being equal, persons who interact with one another tend to like one another." This theorem is one of the most important and most often forgotten in sociology. We often act on the assumption that if we can only "get people together" their coöperation will be improved—other things being equal. What are these other things, and where are they equal? If, among them, we consider only the element of operation, we recognize that two persons who interact with one another tend to like one another only if neither of them behaves,

¹⁸ C. I. Barnard, "On Planning for World Government," in *Approaches to World Peace* (1944), p. 838.

that is, performs operations, so as to irritate the other beyond a certain point. If either of them is irritating, the mere fact of bringing them together, increasing their interaction, will increase negative rather than positive sentiments. In short, interaction and sentiment are mutually dependent in a certain way on the assumption, not that the element of operation is out of the concrete phenomenon, for we know it comes in, but that this element is favorable at a particular point. The same problem reappears, of course, with the other pairs. An effort has been made to face it here as soon as it arose. In mathematics the difficulty is handled under the subject of partial differentiation.

Mutual dependence of sentiment and operation.⁸ Whether we think of the sentiments we share with the savages: fear, hunger, cold, thirst, and the like, or, in more general terms, of those a man brings to any organized group, in either case we say that sentiment gives rise to operations, or is the motive for operations, and that, the operation completed, the sentiment itself is modified. The connection seems to hold good to some degree whether the operation produces a directly useful result or, like magic, takes the place of such an operation which is unknown or impossible. The character of the connection between sentiment and operation is discussed at length in psychology. We know that it is seldom an unconditioned reflex: the farmer does not plow because he is hungry but because it is time to plow. There is no need to go further, once the place of this body of knowledge in the present scheme has been pointed out.

Mutual dependence of operation and interaction. That the members of this pair are mutually dependent is a matter of experience and a truism of the literature of organization. For any set of operations at least as complicated as two men sawing a log, an accompanying set of interactions is required, without which the successful completion of the operations is impossible. In a modern army or mass-production industry the required pattern of interaction is immensely complex, and the interaction is made possible by specialized techniques of communica-

tion. We say that the more elaborate the division of labor the more elaborate must be the process of coordination. Or, as Chapple and Coon write: "The coordination needed in any complex technique is impossible without interaction. As we have seen, most complex techniques involve the activities of more than one person, and, in fact, where people practice a number of complex techniques, extensive interactions must take place to coordinate the work of manufacturing, to secure raw materials, and to exchange the goods produced. In other words, the growth of complexity in technical processes goes hand in hand with an increase in the amount of interaction and in the complexity of the interaction pattern."¹⁰ One more thing should be mentioned. The pattern of interaction is not uniquely determined by the operations. In certain circumstances, there may be several schemes of organization which satisfy equally well the needs of the work to be done. On the other hand, not just any scheme will do. The operations set limits to the pattern of interaction, as indeed the available pattern sets limits to the operations which can be realized. A large amount of learning which it is unnecessary to go into here exists in this field.

Mutual dependence of interaction and sentiment. As far as logic goes, this relationship holds in the primary system, in the sense that if x is a function of y , and y is a function of z , then x must be a function of z . There may even be a direct connection, if interaction with other men, for its own sake, be one of the primary drives of mankind. There is no doubt that the drive exists. Whether we should put it in the same class as hunger and thirst is another question. In any case, this relationship will be considered part of the secondary system, the line between primary and secondary being a matter of convention.

The secondary system. Before any discussion of the ways in which the three elements are represented in the secondary system, some time must be spent looking at this sys-

¹⁰ E. Chapple and C. F. Coon, *Principles of Anthropology*, p. 250.

tem as a whole. The distinction made by Warner between the primary and the secondary, the technological and the social system has been noted. The former is thought of as the initial means of adjustment of a group to its natural environment; the latter controls and regulates the former. This paper has insisted that the idea must be generalized, that any group whatever has a primary scheme of adjustment to the environment on which it operates, be that environment "natural" or not, and that out of the primary system further social relations arise which, for good or ill, profoundly modify the initial adjustment.

The language used has been Warner's but others have made the same distinction. Some idea of a man's behavior being modified by his membership in a group is fundamental in sociology. We are all trying to bring out its full implications. For instance, Barnard writes: "When the individual has become associated with a coöperative enterprise he has accepted a position of contact with others similarly associated. From this contact there must arise interactions between these persons individually, and these interactions are social. It may be, and often is, true that these interactions are not a purpose or object either of the coöperative systems or of the individuals participating in them. They nevertheless *cannot be avoided*. Hence, though not sought, such interactions are consequences of coöperation, and constitute one set of social factors involved in coöperation. These factors operate on the individuals affected; and, in conjunction with other factors, become incorporated in their mental and emotional characters. This is an effect which makes them significant. Hence, coöperation compels changes in the motives of individuals which otherwise would not take place. So far as these changes are in a direction favorable to the coöperative system they are resources to it. So far as they are in a direction unfavorable to coöperation, they are detriments to it or limitations of it."¹⁷

An insight of the same sort comes from

the institutional economists. C. E. Ayres writes: "A component part of every culture is a vast system of tools and tool-using activities. Economists are certainly interested in this sort of thing, and their interest is focused not on the engineering aspect of the tools as artifacts but on the pattern of the system of activities so constituted. Furthermore the interest of economists is not limited to these activities. A further component of every culture is another system of activities in which all these tools and all the products of their use are employed to very curious effect. They are employed ceremonially, and their manipulation in this fashion has the effect of establishing claims, exhibiting prestige, dividing the community in terms of 'ceremonial adequacy' along lines which are more or less coincident with those which are objects of interest to anthropologists, sociologists, political scientists, and the rest. These activities also constitute a system which is part of the total system, which is the culture."¹⁸ Ayres, like Barnard, is making the distinction between the primary and the secondary system. He goes on: "These two activities condition each other in both directions." That is, the secondary system and the primary are mutually dependent. One criticism may be made of Ayres. His line between "tool-using" and "ceremonial" activities stems from Veblen, and from Veblen there remains a hint of disapproval of ceremonial. The secondary system is thought of as a drag on the primary: it is wasteful. Sometimes it may be so, but sometimes the social may help sustain the technological. Barnard's statement is much wiser, that the secondary system may be either a resource or a detriment to the primary. At any rate, the institutional economists have made the same kind of distinction as the sociologists and anthropologists.

Warner, Barnard, and Ayres, writing quite simply, as men must write to give a first impression of a complicated phenomenon, speak as if the secondary system could be separated from the primary. When the

¹⁷ C. I. Barnard, *The Functions of the Executive*, p. 40. See also pp. 45, 52, 60, 120, 286.

¹⁸ C. E. Ayres, *The Theory of Economic Progress* (1944), p. 98.

time comes to refine the theory, a difficulty will have to be faced here. The distinction between the two systems is analytical, conventional if you will, and no more. What we observe are concrete operations, sentiments, interactions. One part of each may be assigned to the primary system and one to the secondary. It would be correct to compare this method with Galileo's description of the path of a projectile in terms of two components: uniform motion in a straight line, and uniformly accelerated motion downward, but for two considerations. In the first place, the physicist who follows Galileo does in fact perform two separate operations. He measures the muzzle velocity of the projectile and the acceleration of gravity. What operations to compare with these do we have in sociology? Only when a new group has been formed to do a particular job have we a chance to watch the secondary system grow out of the primary. In the second place, the two motions the physicist considers are independent of one another. However they be defined, the primary and secondary systems are not independent. Here are the difficulties for someone concerned about the operational definition of concepts, and they are serious.¹⁹ But science proceeds by approximations, and some crude ideas have served well while awaiting refinement. For the purpose of exposition in non-mathematical language, some distinction like that made between the primary and secondary systems seems inescapable.

After these preliminaries, a more detailed discussion of the secondary system can begin. The three determinants of behavior are represented somewhat differently here and in the primary system. Reference to an actual group will illustrate. Roethlisberger and Dickson, in their description of the Western Electric Researches, analyse at length the Bank Wiring Observation Room group.²⁰ In

¹⁹ The author is in full agreement with the operationalists, provided that recognition be admitted as one of the operations, necessary even in reading pointers on dials. "Is that a duck?" is not a meaningless question, and the operation which answers it is recognition.

²⁰ Roethlisberger and Dickson, *op. cit.*, pp. 379-548.

the room men were at work wiring switchboards for central telephone office equipment, and a large amount of their behavior centered around this chief activity. In the present terms, this was the primary system of the group. But another large field of behavior grew out of and elaborated upon the primary system. The group as a whole had adopted a certain standard of output and kept actual output closely pegged to it. Within the group two cliques had developed, that is to say, a pattern of interaction over and above the one required by the work. The members of each clique felt friendship for other clique members and a certain amount of antagonism, consonant with the unity of the group as a whole, for men who did not belong to their clique. Finally each clique followed its own style of operations. In the games its members played, the food they ate, their topics of conversation, each clique was set apart. Even the degree of restriction of output varied slightly but significantly with clique membership. It is unnecessary to go into further details. The charm of the picture is its familiarity.

In the secondary system, then, sentiment is represented by feelings toward persons and their operations: feelings of liking and disliking for individuals, approval and disapproval of the things they do. "Valuations" might be a good word for the sentiments here, since approval and disapproval are not two things different in kind but two values on a continuous scale. Here also should be included the feelings of constraint which may exist between a person in authority and his subordinates. Operations are represented by the ones which do not directly advance the principal activity of the group but which are, as we say, social: expressions of group membership and differentiation. Finally, the pattern of interaction is more than the one required for the coordination of the operations of the primary system.

Mutual dependence of interaction and sentiment. The aim of this paper is to outline a conceptual scheme and not to elaborate the theorems which may be stated in its terms. On the other hand, there is little point in developing the scheme without showing the

uses to which it may be put. In fact the theorems, in the inchoate form, probably suggest the scheme, although any scheme worth its salt, once developed, will suggest further theorems. The elements of social behavior are mutually dependent in the secondary system as in the primary, and the chief theorem of the mutual dependence of interaction and sentiment has already been cited, namely that, speaking relatively, you like persons you interact with frequently and dislike persons you interact with infrequently. Thus, in the Bank Wiring Observation Room, friendships were positively associated with clique membership. The mere fact is the important thing, no matter which determinant you choose to regard as the "cause" and which as the "effect." The theorem seems to be fundamental in sociology and assumed in much of our discussion of the in-group.

The theorem, like all such theorems, does not hold good unless "other things are equal." One of these other things, already mentioned, is the element of operation. Another is authority. If one man is interacting with another and is his superior in authority, experience suggests that new sentiments are often aroused which make the emotional relationship deeply ambivalent, and for perfectly good reasons in that in fact two influences are at work: the interaction and the authority. The latter may cut down the amount of interaction which would otherwise be expected.²¹

Mutual dependence of sentiment and operation. Two kinds of mechanism are represented here, both familiar. The first is similarity and difference. You approve of behavior which is like your own and disapprove of behavior which is different. On the other hand, if you like a certain form of behavior, your own will tend to conform to it. In the Bank Wiring Room, each clique was inclined to ridicule the behavior of the other. One clique even thought that its topics of conversation were more refined. The second mechanism resembles the relationship

between sentiment and operation in the primary system in that sentiment is the motive for operations. If you feel liking or disliking for a man you tend to express the sentiment in operations. In primitive societies these operations become elaborate, in gift exchanges which may practically take over the distribution of goods, but they are important also in our own society.

Mutual dependence of operation and interaction. You increase interaction with persons who perform the same kind of operations that you do, and decrease interaction with those who do not. Roethlisberger and Dickson point out that in the Bank Wiring Room the output of the members of one of the cliques was distinctly below the standard of the group as a whole: "But, it may be asked, did their low output determine their position in the group, or did their position in the group determine their output? The answer is that the relation worked both ways; position in the group influenced output, and output influenced position in the group. In other words, these two factors were in a relation of mutual dependence."²² The men in question were members of an excluded clique (interaction) because their output was low (operation), but it was also true that their output was low because they were members of an excluded clique. Here the relationship has been described in terms of interaction and operation alone. In their word "position," Roethlisberger and Dickson seem to include a reference to sentiment as well. Not only was interaction with the clique low but its behavior was given a low value. One purpose of the present conceptual scheme is to break down words like "position" into the simpler elements which we actually observe.

Another example of this relationship is given by the social climber. When he wants to enter a new group, he will model his behavior on the characteristic pattern of the group. He assumes that in the measure that he adopts the pattern, the members will increase interaction with him. Furthermore, since the relationship is always one of

²¹ Much of the great book of J. L. Moreno, *Who Shall Survive?* (1934), is devoted to the mutual dependence of interaction and sentiment.

²² Roethlisberger and Dickson, *op. cit.*, p. 520.

mutual dependence, the more he is able to interact with the members, the more likely he will be to copy their behavior faithfully. To pass on to another mechanism, many of the operations men perform merely serve as occasions for social interaction. In the Bank Wiring Room, many of the operations, such as playing certain kinds of games, which were part of clique behavior patterns, led to increased interaction between the members of each clique.

There is no need to labor the point that these types of behavior are so common as to be banal. It may be that the principles of human society, though many enough, are fewer than we have been ready to admit. In many sciences the principles are less complicated than their interweaving in the concrete situation. For each case of mutual dependence among the elements of behavior, enormous complexities are introduced when, instead of considering two individuals or groups, you consider three or more. Thus it is proverbial that increased interaction and positive sentiment between two persons in the in-group implies decreased interaction with and negative sentiment toward third persons who are outsiders. A more general statement is that the relationship between two individuals or groups A and B is not something apart from the relationships between A and C, D, E . . . and those between B and C, D, E . . .²³ Elaborate systems of relationships arise in this way. Perhaps the best known of these systems (and note that authority is always one of the factors in them) are the family organizations of primitive and civilized societies, but the same principles apply generally.²⁴

Dependence of the secondary system on the primary. The primary and secondary systems have been described separately although they are not separate. Not only can the two be divided only for analysis but, even in analysis, they are not independent of one another. The secondary system arises,

so to speak, out of the primary and in turn reacts upon it. Barnard's remark is worth repeating: "Coöperation compels changes in the motives of individuals which otherwise would not take place. So far as these changes are in a direction favorable to the coöperative system they are resources to it. So far as they are in a direction unfavorable to coöperation, they are detriments to it or limitations of it." As usual, the important thing is to recognize the mutual dependence of the primary and secondary systems explicitly, systematically, and in its full generality.

Here the dependence of the secondary system on the primary will be considered first, and special emphasis will be given to the element of interaction. Operations in the primary system may, within limits, demand a certain scheme of interaction. In industry a number of men may be working in the same room. Or they may be performing in a certain order different parts of a total operation on an object, so that when one man has done his part he passes the object on to the next man. Or they may have the same foreman. Whatever the reason, geography, flow of work, or supervision, they are, as we say, thrown together. What they do makes it likely or inevitable that they will interact. Furthermore, interaction left to itself increases positive sentiment, which will increase the interaction still more. This last mechanism has been arbitrarily called part of the secondary system, so that, in terms of the present conceptual scheme, the primary system gives rise to the secondary. There is another way of saying the same thing. When men interact in the primary system, it is often observed that they increase their interaction beyond the amount required by the primary system. We call this increment social, and say that any congeries of individuals, brought together in any way, tends to become something more, a social group.

Another mechanism by which the secondary system elaborates on the primary is the following. In the primary system there is usually in every group a man who acts as center of communications, that is, as leader of the group, and in complex organizations these centers are arranged in a hierarchy.

²³ Stated in E. Chapple, *Measuring Human Relations*, p. 79, but the present writer does not accept Chapple's method of describing the relationships.

²⁴ See R. Firth, *We, The Tikopia* (1936), chs. IV-VI, also Roethlisberger and Dickson, *op. cit.*, p. 333.

The leaders receive information and give orders, and it seems to be a matter of experience that between persons in authority and their subordinates a certain kind of sentiment often exists. It may be called constraint, but under any name the sentiment between two men in this relationship is different from the sentiment between two men who are interacting as equals. Furthermore, between such men, interaction, instead of increasing, tends to be kept near the minimum required by the primary system. This theorem is warranted by the behavior of fathers and sons in many patriarchal families and by the separation between grades in armies. It is subject to the usual limitation of "other things being equal." In particular, if the subordinate is in an insecure position and feels that his advancement depends less on his own work than on his personal relationship with his superior, bootlicking may begin and interaction increase rather than decrease.

Once again, then, the requirements of the primary system give rise to certain sentiments between men, sentiments which have been called part of the secondary system. In this particular case, the sentiments tend to discourage rather than encourage social interaction. The increase of interaction between so-called equals and its relative decrease between superiors and subordinates are the first steps toward the formation of classes in organizations and in society at large.

Dependence of the primary system on the secondary. Barnard argues that once the secondary system is established its influence on the primary may be favorable or unfavorable, and Roethlisberger and Dickson make the same point: "It is well to recognize that informal organizations are not 'bad,' as they are sometimes assumed to be. Informal social organization exists in every plant, and can be said to be a necessary prerequisite for effective collaboration. Much collaboration exists at an informal level, and it sometimes facilitates the functioning of the formal organization. On the other hand, sometimes the informal organization develops in opposition to the formal organization."²⁵ An example of the latter is the

restriction of output adopted through informal organization.

Another example has been made famous by Veblen and referred to in the passage from Ayres cited above. Two parts may be distinguished in any economic activity such as building a house. The form of the house is in part determined by "needs" for such things as warmth and shelter. It is in part, we usually say in its style, determined by other factors, by the social class to which its occupants belong and by more general community custom. Veblen spoke of the effect of these latter factors as "conspicuous expenditure:" they brought about a destruction of wealth. In the present terms, the effect of the secondary system is in some way unfavorable to the primary. Conspicuous expenditure is particularly conspicuous at times when the classes are fluid, that is, when people feel a need to make their social position highly visible.

What Veblen and his followers forgot is that if y is a continuous function of x , there is probably a region within which the value of the function is positive not negative. The suggestion has been made that well established social standards of living, which require that families purchase certain goods as a mark of group membership, may help prevent the collapse of the economic system in times of crisis, by sustaining demand.²⁶ At any rate, the main lesson of the industrial research of the last two decades is that effective coöperation is never a matter of the primary system alone, and that developments in the secondary system may either sustain coöperation or break it down.

Finally, the secondary system of a social group may give rise to the primary system of another social group. This happens when the standard of operations in the secondary system becomes a positive program which is to be put into effect and thus requires organization. For example, antagonisms in an industrial plant may lead to the formation of a union. But a union is an organized activity which will have its own primary and second-

²⁵ E. Mayo, "La Stabilité Économique et le 'Standard of Living'" in *Le Travail Humain*, Vol. I (1933), pp. 49-56.

²⁶ Roethlisberger and Dickson, *op. cit.*, p. 559.

ary systems. In fact the leaders of the union may have the same kind of trouble with their followers that the managers of the factory had originally. The systems are always to be discerned with relation to the particular group in question.

The condition of equilibrium. The elements of social behavior have been described as mutually dependent in two systems, which are themselves mutually dependent in the total social system. Experience in the developed sciences, and, as will be shown, even in our own, suggests that the relationship between the elements is not determinate without some criterion of equilibrium. As Henderson puts it: "Another characteristic of many ideal systems that is, in general, indispensable in order that conditions shall be determinate is the establishment and use of some definition of equilibrium, whether in the case of statical equilibrium or in the case of dynamical equilibrium. For the abstract conceptual scheme this is as a rule the decisive feature that goes farthest to establish determinate conditions."²⁷ In a developed science, a general equation of equilibrium will appear which takes its place as one of the equations, equal in number to the number of variables, which describe determinate conditions.²⁸

Henderson is writing about an ideal system such as the social system of this paper pretends to be. This kind of system can hardly be set up unless it is provisionally treated as isolated, in the sense that exchanges with whatever constitutes its environment have the value 0 or some other known value. The criterion of equilibrium applies particularly to such a system. Now nothing is in fact isolated from the rest of the universe, nor are all the exchanges between a system and the rest of the universe known. Nevertheless Newton and Gibbs showed that some systems can without serious difficulty be treated as isolated, even when the influences of the environment are great. As Henderson goes on to say: "With the help of estimates of disturbances intro-

duced from without and of other disturbances that result from actions in the opposite direction, even when such disturbances are very complex, much can often be accomplished when the characteristics of the ideal isolated system are known."²⁹

All this is general, a matter of the experience of the older sciences in stating their findings. What is its application to sociology? The crucial question is how far any social system, or, more simply, any group can be treated as isolated without irreparable violence being done to the facts. In the present ideal system, the effects of the environment and of plant and tools are regarded as given in any particular instance. As for concrete systems, primitive tribes, independent national states, and business enterprises in competition with other enterprises may come closest to realizing the conditions for being described as isolated systems. Exchanges with the environment are most nearly determinate. The difficulties are great, yet it is likely that we must, whether or not social systems shall be treated as isolated, adopt some definition of equilibrium. In fact we have done so already. In or out of business, we cannot escape the idea of the "going concern."

An example follows. The emotional constraint between superiors and subordinates is often one of the forces in a social system. Its importance is particularly clear in societies where the family is the unit which performs the essential operations on the environment. Here the father, or, in matrilineal groups, the mother's brother is the supreme authority and the sentiments existing between him and his subordinates, the other family members, help determine many of the other emotional relationships in the family. Similar observations could be made of other kinds of social groups. Now the thing to remember here is that the sentiment between superior and subordinate does not depend on the mere fact that the superior gives orders. Just giving orders has quite different effects. It depends on the giving of orders which will be obeyed—a truism whose implications we neglect at our peril. For the

²⁷ L. J. Henderson, *Pareto's General Sociology* (1935), p. 85.

²⁸ For illustrations, see E. Mach, *The Science of Mechanics*, pp. 72-5.

²⁹ L. J. Henderson, *op. cit.*, p. 83.

willingness of the members of an organization to obey orders, to accept authority, depends in a complicated way on the working of the organization as a whole. The operations in the primary system must provide a surplus of means of inducing the members to cooperate, and the secondary system must not generate too much antagonism to destroy cooperation in the primary. Of families as of other groups we may say that the organization can maintain itself only if its orders are obeyed, or, alternatively, that only if it maintains itself will its orders be obeyed. In either case, whether we like it or not, we are using a criterion of equilibrium and assuming, like the other sciences, that a particular condition, here an emotional relationship, is determinate only when equilibrium exists. There must be a large number of other conditions in social systems which are determinate only if the systems are going concerns. The greatness of Barnard's book, *The Functions of the Executive*, lies in the fact that, almost alone among sociological treatises, it insists, and keeps insisting, that in the case of equilibrium the amount by which any factor in social organization can be altered without altering all the other factors is strictly limited. The most important things about social groups, the conditions of their life and death, are the ones we study least.

In *An American Dilemma* Myrdal argues that the idea of equilibrium is conservative.³⁰ Of course some simple-minded notion of statical equilibrium could be used to justify the existing order. The best corrective is not to abandon the idea, which we use whether we like it or not, but to become familiar with its actual employment in the sciences. The fundamental equation of mechanics asserts that for equilibrium the variation of the work done in the system is 0. Alternatively, if the system is to pass from an initial configuration to a different final configuration, work will have to be done on the system. In ordinary language, this statement does not claim that change is impossible; it only defines the condition, namely

the accomplishment of work, under which change is possible. If the work is done, Myrdal's principle of cumulation may come into play, depending on the conditions and constraints of the system, and the system pass rather rapidly to a new configuration. But these matters of logic have nothing to do with conservative or radical political opinions.

Rate of change may be more significant than change itself. Any conceptual scheme in sociology must be equipped to deal with dynamics, with change in the social system in respect to time.³¹ In the broadest sense, the scheme must be historical. If the present one has been described for the statical case, a method which always makes exposition easier, it is not meant to be limited to that. It asks: If one of the elements, or one of the systems, is changing in a certain way, at a certain rate, what kinds of changes may be expected in the others? The idea of equilibrium suggests that it is more illuminating to study even a stable situation in terms of change than change in terms of a stable situation. Social scientists used to talk about the "tyranny of custom." Nothing is more defenseless than a custom, alone. Not single customs but systems of custom survive. For instance, gift exchange in primitive and modern societies have been cited as evidence that we show our sentiments in operations. But any recollection of Christmas proves that we do not give presents to people just because we like them. We also think of what might happen if we did not give the presents. They have ramifications. For the normal situation, both the specific force and the complex of forces are present at the same time. This should be obvious, but apparently is not so to everybody.³² We can best account for the survival of any system by noticing what happens when a change is introduced in one of the components. At Christmas, at least, our ordinary social thinking follows the method of science.

³⁰ See C. Arensberg, "Industry and the Community," in *The American Journal of Sociology*, Vol. 48 (July, 1942), reprinted in S. D. Hoslett, ed., *Human Factors in Management* (1946).

³² For an example, see G. C. Homans, "Anxiety and Ritual" in *American Anthropologist*, Vol. 23 (1941), pp. 164-72.

³¹ G. Myrdal, with the assistance of R. Sterner and A. Rose, *An American Dilemma* (1944), Vol. 2, p. 1055. See also Appendix 3.

INTERNAL MIGRATION IN PEACE AND WAR*

HENRY S. SHRYOCK, JR., AND HOPE TISDALE ELDRIDGE

Bureau of the Census

IN A DISCUSSION of wartime migration, it is well to remind ourselves that Americans are a very mobile people in peacetime. The long and frequent treks of the frontier period seem to have left us with a national habit of pursuing opportunity about the country. The census data on State of birth have recorded the cumulative effects of these currents and cross-currents at decennial intervals since 1850. Not until 1940, however, did we collect national figures on internal migration among and within States during a specific period of time. Data from which to derive measures of internal migration during the war period are relatively scarce. However, by piecing together the various items of information that are available, one can obtain the broad outlines of recent population movements in the United States.

The 1940 data cover the period from April 1, 1935, to April 1, 1940, a period characterized by gradual and partial recovery from the Great Depression. Cityward migration from rural areas was accelerating but still lagged behind the pace set in the booming twenties. The centrifugal scatter from metropolis to suburbs was more pronounced than ever. The long-established exodus from the South to the North was slackening, but the older westward movement went on, aggravated by drought in the Plains States.

About 14,000,000 persons lived in a different county¹ in 1940 from their county of residence in 1935. It is they whom we have somewhat arbitrarily chosen to call "mi-

grants." These 14,000,000 do not represent the gross migration during the 5-year period. Additional people left a county but returned within the period, some migrants died, and others were born after 1935. Nonetheless, it is an impressively large figure. Wartime migration was still larger. The results of a sample survey taken in March, 1945, show that about 15,000,000 civilians were living in a different county in 1945 from their county of residence at the time of Pearl Harbor (December 7, 1941).² Thus, in three years and three months more civilians had moved from one county to another than in the whole five years between 1935 and 1940.

REGIONAL SHIFTS

Let us consider next the interchanges among the four broad regions. The three flow charts in Figure 1 show migration among the four major regions during the war and during two earlier periods. The first map, based on a sample survey, shows interregional movements of the civilian population for the period from Pearl Harbor to March, 1945. The second map shows movements for the period 1935 to 1940. Map 3 shows the "lifetime migration" of the 1940 native population. This last is based on a comparison of region of residence in 1940 with region of birth and gives some historical perspective against which to study the first two maps.

On each map, the width of the arrows represents the movement from one region to another as a proportion of all interregional migration during the period. This scheme demonstrates the relative importance of movements from one region to another in a given period and also changes in relative importance from one period to another.

It is apparent at once that regional shifts

* Adapted from a paper read before the American Sociological Society at Cleveland, Ohio, March 1, 1946. The authors wish to acknowledge the technical assistance of Miss M. Frances Taylor in the preparation of the charts and maps.

¹ The figure given here excludes persons who moved between cities of 100,000 or more and the remainder of their counties, who were also classified as migrants in the 1940 census reports on internal migration. The number of these is estimated at 1,500,000 on the basis of a sample tabulation.

² U. S. Bureau of the Census. Population—Special Reports. Civilian migration in the United States: December, 1941, to March, 1945. Series P-S, No. 5, Sept. 2, 1945.

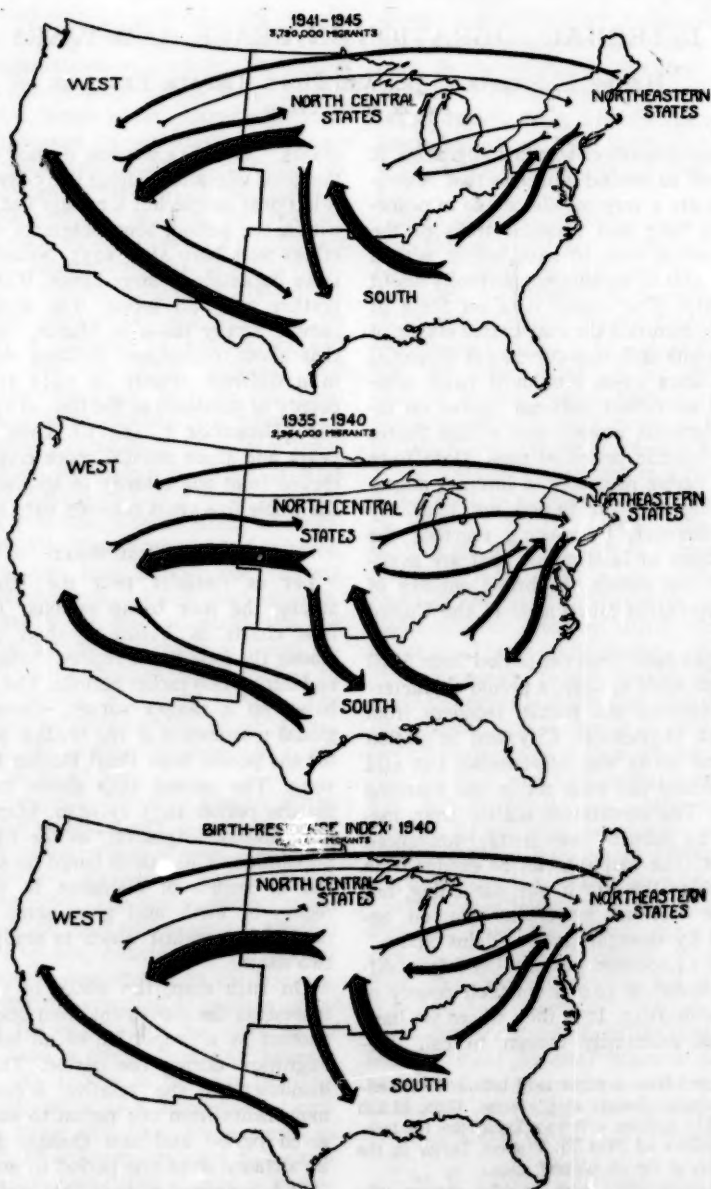


FIGURE 1. Interregional migration in the United States

during the war were similar to those during the preceding five years, as well as to the long-term shifts of the native population. The most noticeable changes are the increase in relative importance of South-to-West migration and the decline in the importance of migration between the Northeastern States and the North Central States. Throughout, movement to and from the Northeast was relatively unimportant, but movement from the North Central States to the West was of prime importance. There was some tendency for the movement from the West to increase in importance.

During the war period, the heaviest movement of all with reference to a single region was that from the South to other regions. During the two earlier periods, movement from the North Central States to other regions was the largest. This interchange in rank is probably associated with the end of the depression and the acceleration of out-migration from the South.

Changes in the volume of movement are of particular interest with reference to the war period. The total number of persons whose region of residence in 1940 differed from their region of residence in 1935 was almost 3,000,000. The total number of civilians whose region of residence in 1945 differed from their region of residence at the time of Pearl Harbor was 3,800,000.

Another comparison that can be made at the regional level is in terms of the relative amount of mobility within the several regions. Intraregional migration (i.e., migration between States within a region and migration between counties within the States of a given region) was considerably greater during the war than before the war (about 11,600,000 in each period but the wartime period was $3\frac{1}{4}$ years whereas the prewar one was 5). This migration was on the whole more important in the South (12.2 per cent of the total civilian population in 1945) than in any other region. It was almost equally important in the West (11.8 per cent of the 1945 civilian population).

Between 1935 and 1940, there were contrasts in the regional shifts of whites and nonwhites. The Northeast had a net out-

migration of 111,000 whites to the South, but a net in-migration of 39,000 nonwhites (mostly Negroes) from the South. The turning of the tide along the Atlantic seaboard is a new phenomenon in which the District of Columbia, Maryland, Virginia, and Florida have been the chief gainers of white population and New York City has been less the mecca than formerly. The movement between the South and the North Central States was somewhat similar, the net gain of 55,000 by the latter consisting of 48,000 nonwhites and only 7,000 whites. For the white, as for the total population, the West was the only region to show a gain from the rest of the country. For the nonwhite population, however, the South lost a net total of 107,000 persons, and each of the other three regions gained through interregional migration. Indications are that in the period 1941 to 1945 there may have been a still larger out-migration of nonwhites from the South, since the estimated total number of nonwhite civilian interstate migrants was considerably larger for this period than for the period 1935 to 1940.

SPECIFIC MIGRATORY STREAMS

An elaborate tabulation of the data on internal migration was required to cross-classify residence in 1940 by residence in 1935 for every State. It is only by such detailed cross-classification that specific streams of migration can be discovered. Two examples are shown in Figures 2 and 3. One map shows the distribution by State of origin of the migrants into California, an area characterized by heavy in-migration. The other shows the States of destination of migrants from Oklahoma, an area characterized by heavy out-migration. Despite the great distance between them, the 95,000 migrants from Oklahoma to California exceeded the number moving between any other two States. Clearly, the publicists were correct in dramatizing this particular stream.

Altogether, each of 26 States contributed 10,000 or more migrants to California. Texas, Missouri, and Illinois ranked next after Oklahoma.

The usual direction of the 309,000 out-

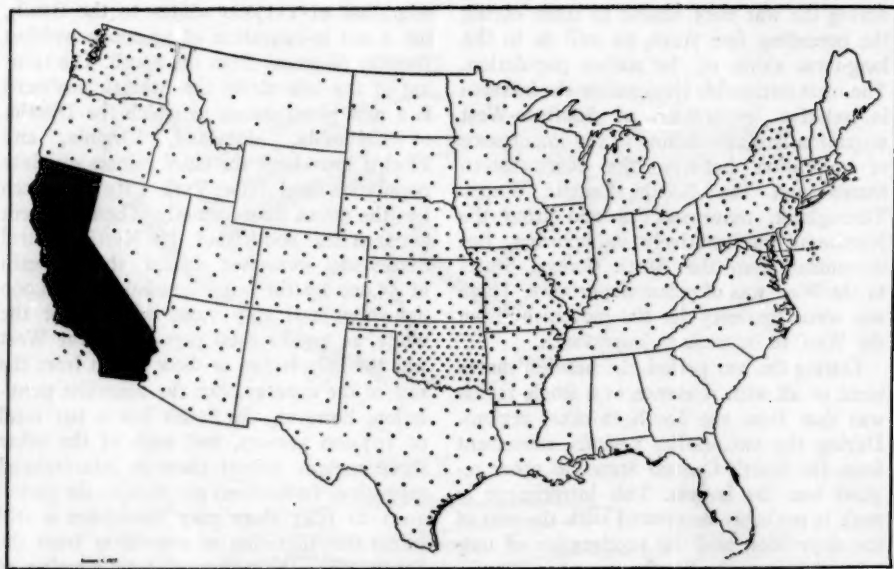


FIGURE 2. In-migrants to California, 1935 to 1940, by State of Origin (One dot equals 1,000 migrants)

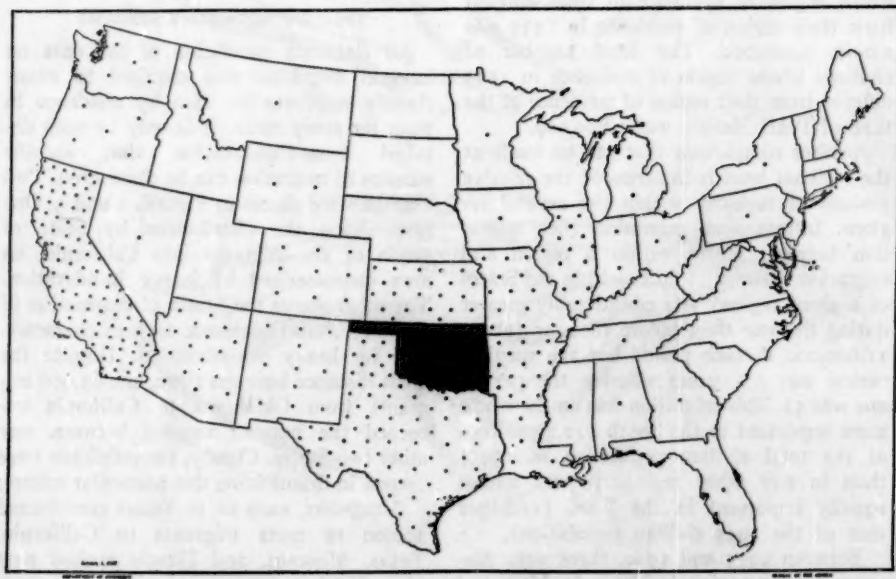


FIGURE 3.—Out-migrants from Oklahoma, 1935 to 1940, by State of Destination (One dot equals 1,000 migrants)

migrants from Oklahoma was westward. Of the 8 States attracting 10,000 or more of these, all but Illinois, Arizona, and California were contiguous to Oklahoma.

Although the long-distance streams of migration into California between 1935 and 1940 were the most spectacular, there were many other important ones. There were 35 other cases in which 10,000 or more persons moved from a given State to a noncontiguous State.

The size of the streams of migration below the regional level is not known for the war period. Only the net gain or loss through migration to the civilian population of States between April, 1940, and November, 1943, has been estimated.³

Little has been said so far about migration between urban and rural areas or to and from specific cities. This omission is in deference to a marked bias in the data on place of residence in 1935. There was a pronounced and, in retrospect, understandable tendency for people to report a city as their previous residence even when they lived outside its corporate limits. This tendency is important enough to invalidate the indicated net out-migration from urban areas between 1935 and 1940. We can, however, draw some conclusions from the statistics on urban-rural residence in 1935, and the characteristics of migrants in the reported streams are probably similar to those of migrants in the actual streams. Moreover, figures on the urban-rural destinations of in-migrants (being places of residence observed in 1940) should be reliable.

It can readily be demonstrated that the migration to California tended to shift people from farm to nonfarm residences. Of California's in-migrants, 15.4 per cent reported themselves as living on farms in 1935. For the reasons just given, this is probably a minimal proportion. But only 8.1 per cent were settled on farms in 1940. Again of those who came from rural farms, about one-quarter moved to rural farms in Cali-

fornia. About 13 per cent of the "Okies" who moved to California between 1935 and 1940 settled on rural farms as contrasted with at least 35 per cent who had been on farms in Oklahoma. The deruralizing effect of migration is also shown by data from the March, 1945, survey. The movement of the 11,810,000 intercounty migrants 14 years old and over resulted in a net loss of 820,000 persons to the farm population.⁴

MIGRATION BY SEX

During the period from 1935 to 1940, the difference between the over-all migration rates of males and females was small. There was a tendency in the white population for the proportion of males among migrants to increase with distance spanned—the percentage of males was highest among migrants between noncontiguous States. Because job opportunities tend to be relatively most plentiful for women in cities and relatively least plentiful on farms, migration between urban and rural areas has resulted in a female majority in the urban population and a male majority in the rural population, particularly in the rural-farm population. Between 1935 and 1940, only 48 per cent of migrants from rural-farm areas to urban areas were males as compared with 54 per cent of migrants from urban areas to rural farms.

In most cases, the proportion of males among the migrants from one region to another was higher than the proportion in the total population of the region of either origin or destination. The single exception occurred in the migrant stream from the South to the Northeast; the proportion of males was nearly 50 per cent in both regions, but only 48 per cent among the migrants. Relatively attractive openings for Negro women in domestic service in Northeastern cities may explain the exception. At the other extreme, 56 per cent of the migrants from the West to the South were males, although the percentage in the West's population was but

³U. S. Bureau of the Census. Population—Special Reports. Interstate migration and other population changes: 1940 to 1943. Series P-44, No. 17, August, 28, 1944.

⁴U. S. Bureau of the Census. Population. Shifts in farm population: December, 1941, to March, 1945. Series P-S, No. 5, October 29, 1945.

51. Sex differences among the interregional streams of migrants were rather small, however. More extreme differences were found in smaller areas and in smaller streams of migration. For instance, the population of New York City in 1940 was 49 per cent male and 46 per cent of its in-migrants were males. However, of the 6,100 persons (mostly nonwhite) who moved from North Carolina to New York City, only 38 per cent were males. As a contrasting example, 57 per cent of the persons who left New York City during the 5-year period for rural-farms in New York State were males. These are extreme cases, but by no means the most extreme.

During the war, civilian migration was predominantly female as a result of the exclusion of millions of young males, those in the most migratory ages, from the civilian population. Only about two-fifths of all civilian migrants were males. The proportion was as low as one-fifth at ages 20 to 24, but above age 45 male migrants roughly equalled female migrants. Females predominated especially in the interstate moves, possibly because of long moves by wives of servicemen.

MIGRATION BY COLOR

The percentage of migrants in the nonwhite population in 1940 was 8.5, as compared with 12.3 per cent in the white population. The rate was lower for nonwhites than for whites for migration within States, between contiguous States, and between noncontiguous States. There was a greater difference between native and foreign-born whites than between whites and nonwhites. The migration rate was 12.4 for native and 7.2 for foreign-born whites.

Data for the prewar quinquennium show that nonwhites made up very high proportions of some particular streams of migration. These high proportions reflect not only the relatively large numbers of nonwhites in the areas of out-migration but also highly "favorable" combinations of push and pull factors. Only 14 per cent of all out-migrants from the South between 1935 and 1940 were nonwhites, but 34 per cent of those moving

from the South Atlantic to the Middle Atlantic States were nonwhites, as were 20 per cent of those moving from the East South Central to the East North Central States. More specifically, there were extremely high proportions of nonwhites in the following streams:

	<i>Per Cent</i>
South Carolina to New York State	84
South Carolina to Pennsylvania	80
Rural-farm South Carolina to New York City	94
Rural-farm South Carolina to Philadelphia	96
Mississippi to Illinois	78
Mississippi to Michigan	72
Rural-farm Mississippi to Chicago	93
Rural-farm Mississippi to Detroit	85

Negro females predominated in these movements.

There is little information on migration of nonwhites during the war. In 1945, the percentages of migrants in the white and nonwhite civilian populations 14 years old and over were about the same, however. Available evidence seems to indicate that migration of nonwhites, like that of females and of families, lagged behind in the early part of the defense period but caught up later. There is some indication that migration of Negroes to the West Coast increased during the war. The Negro population of the five congested production areas in the West Coast showed an increase of 213 per cent between 1940 and 1944.⁵

MIGRATION BY AGE

The variation of the percentage of migrants with age for the period 1935 to 1940 is shown in Figure 4.⁶ In the white population the percentage of migrants starts off at 13.1 for children 5 to 13 years old. It then dips to 10.7 for those 14 to 17 years old. Most of the children in both groups must have moved with their parents. Families are

⁵ U. S. Bureau of the Census. *Characteristics of the population, labor force, families, and housing: Congested Production Areas. Series CA-3. July 20 to Oct. 7, 1944.*

⁶ U. S. Bureau of the Census. *Sixteenth Decennial Census. Population. Internal Migration, 1935 to 1940: Age of Migrants.* Washington, Government Printing Office, 1946.

fairly mobile when children are young; in fact, as the child leaves babyhood many parents leave apartment houses for suburban single-family homes (often crossing a county line in the process). By the time children reach adolescence, the family is kept from moving by home ownership, established jobs, and school ties. The tabulated age groups are not ideal for exhibiting this pattern. Non-white children 14 to 17, however, have about

average migrant moved $2\frac{1}{2}$ years before 1940, we may estimate that maximum mobility occurred roughly at age 25. From this peak the proportion of migrants fell off regularly and was only 6.7 per cent for persons aged 65 years and over.

Very nearly the same profile is found for each sex and for each type of migration (within State, etc.). The proportion of migrants was greater for females than for

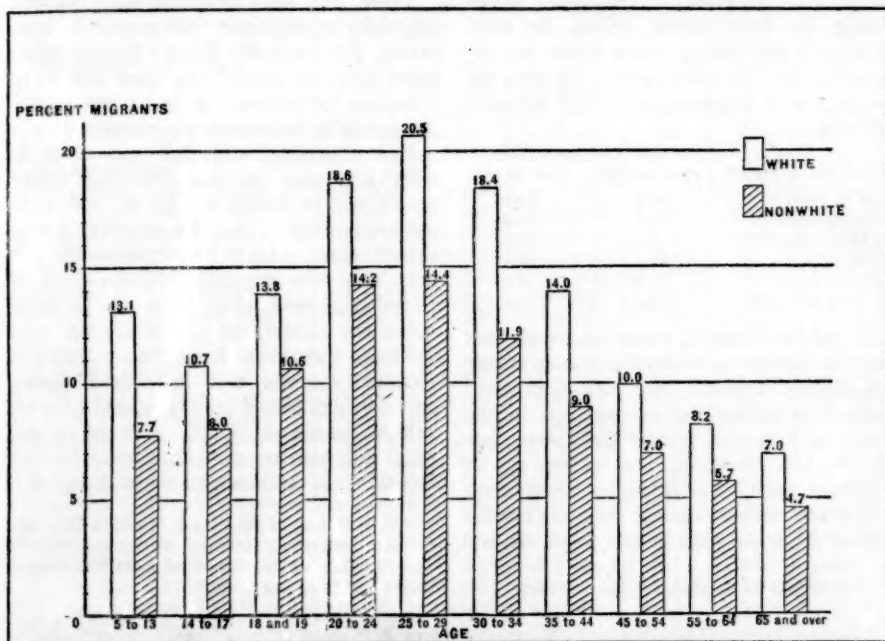


FIGURE 4. Per cent who were migrants in each age group, for the United States, White and Nonwhite: 1940

the same migration rate as those 5 to 13, possibly because a larger proportion of them than of whites live on farms and because a smaller proportion of Negro families have the economic resources to make the adjustment just described. Early marriage and early entry into the labor force are other possible explanations.

The proportion of migrants rose regularly to a peak of approximately one-fifth in the group 25 to 29 years old. Assuming that the

males in the age range from 14 to 24, probably reflecting their earlier age at marriage. This directional difference existed only among the migrants moving the shorter distances. It would be interesting to be able to differentiate between migration in search of a job and migration caused by marriage, widowhood, and orphanhood.

The median age of migrants in 1940 was 28.6 years. For comparability we should use the median age of nonmigrants 5 years old

and over. This is 32.2 years, or about $3\frac{1}{2}$ years older.

The prewar and wartime migration rates by age, are compared in the table below. In view of the fact that the first period was five years long and the second only three and one-quarter, it may safely be said that the migration rate was greater in each age group during the war. The difference was greatest for persons 20 to 24 years old despite the exclusion of members of the armed forces during the later period. (Since the total number of intercounty moves is not known, it is felt that it is not proper to compute the percentage of migrants per annum for each period.)

Age at end of period	Per cent who were migrants	
	1935 to 1940	1941 to 1945
5 to 13 years ¹	12.4	14.7
14 to 19 years	11.4	11.9
20 to 24 years	18.1	23.2
25 to 44 years	16.4	16.4
45 to 64 years	9.0	8.0
65 years and over	6.9	5.6

If children under 5 years old are omitted from consideration, internal migration among urban, rural-nonfarm, and rural-farm areas tended to reduce the average age in the receiving area and to raise the average age in the area of origin. The median age of migrants from rural farms to urban areas (25 years) differed sharply from the median age of the urban population 5 years old and

over (33 years). Similarly, we find that interregional migration tended to lower the median age of the receiving region and to raise that of the region of origin. The South was a partial exception. The median ages of its in-migrants from the other three regions exceeded that of its relatively young total population.

MIGRATION AND EDUCATION

There has been much interest in the selectivity of migration with respect to education. For 1940 the Census Bureau tabulated years of school completed and 1935 residence for persons 25 to 34 years old.^a These adults had nearly all completed their formal education, and their age range is sufficiently small so that important biases should not be introduced by differences in age composition. There is a remarkably consistent direct association between the per cent who were migrants and the years of school completed. (See Table 1.) To point out a few classes, the percentage was 13.3 for those completing fewer than 5 grades of elementary school, 14.2 for 7 or 8 grades, 21.1 for high school graduates, and 36.4 for college graduates. Thus, there is an accelerating increase that culminates in over one-third of college graduates being mi-

^a U. S. Bureau of the Census. Sixteenth Decennial Census. Population. Internal Migration, 1935 to 1940: Social characteristics of migrants. Washington, Government Printing Office, 1946.

TABLE 1. PERCENT DISTRIBUTION BY MIGRATION STATUS OF PERSONS 25 TO 34 YEARS OLD, BY YEARS OF SCHOOL COMPLETED, FOR THE UNITED STATES: 1940

Years of School Completed	Total	Non-migrants	Migrants			Immigrants	Migration Status Not Reported
			Total	Within a State	Between States		
Total.....	100.0	79.6	18.8	10.5	8.3	0.5	1.1
Grade School: Less than 5 years ¹	100.0	82.2	13.3	8.7	4.6	0.5	3.9
5 and 6 years.....	100.0	85.0	13.6	8.3	5.3	0.3	1.1
7 and 8 years.....	100.0	84.5	14.2	8.3	5.9	0.4	0.9
High School: 1 to 3 years.....	100.0	81.1	17.7	10.2	7.5	0.4	0.8
4 years.....	100.0	77.6	21.1	11.5	9.5	0.5	0.8
College: 1 to 3 years.....	100.0	69.0	29.5	15.5	14.1	0.6	0.9
4 or more years.....	100.0	61.5	36.4	17.8	18.6	1.1	0.9

¹ Includes those not reporting on years of school completed.

TABLE 2. IN-MIGRANTS, OUT-MIGRANTS, AND NET MIGRANTS 25 TO 34 YEARS OLD IN 1940 BY YEARS OF SCHOOL COMPLETED, AND RATES OF MIGRATION, FOR REGIONS: 1935 TO 1940
(Those not reporting on years of school completed included in less than 5 years of grade school.
Percent not shown where less than 0.1)

Region and Years of School Completed	In-migrants	Out-migrants	Net Migrants	Percent of Corresponding 1940 Population Group		
				In-migrants	Out-migrants	Net Migrants
<i>Northeastern States</i>						
Grade School: Less than 5 years...	4,678	2,929	+ 1,749	2.0	1.3	0.8
5 and 6 years.....	6,115	3,751	+ 2,364	1.9	1.2	0.7
7 and 8 years.....	21,310	21,961	- 651	1.1	1.1	—
High School: 1 to 3 years.....	19,087	26,538	- 7,451	1.5	2.0	- 0.6
4 years.....	26,598	40,647	- 14,049	2.0	3.1	- 1.1
College: 1 to 3 years.....	14,910	20,727	- 5,817	4.3	5.9	- 1.7
4 years or more....	26,114	32,612	- 6,498	6.2	7.8	- 1.6
<i>North Central States</i>						
Grade School: Less than 5 years...	8,544	5,292	+ 3,252	5.3	3.3	2.0
5 and 6 years.....	11,676	7,101	+ 4,575	4.8	2.9	1.9
7 and 8 years.....	45,347	58,820	- 13,473	2.3	3.0	- 0.7
High School: 1 to 3 years.....	35,678	58,404	- 22,726	2.5	4.1	- 1.6
4 years.....	43,808	92,752	- 48,944	2.6	5.6	- 3.0
College: 1 to 3 years.....	22,352	44,992	- 22,640	4.3	8.7	- 4.4
4 or more years....	26,344	45,831	- 19,487	7.2	12.5	- 5.3
<i>The South</i>						
Grade School: Less than 5 years...	5,334	16,314	- 10,980	0.5	1.5	- 1.0
5 and 6 years.....	6,654	23,530	- 16,876	0.6	2.3	- 1.6
7 and 8 years.....	29,395	74,196	- 44,801	1.8	4.6	- 2.8
High School: 1 to 3 years.....	35,719	54,932	- 19,213	2.8	4.3	- 1.5
4 years.....	53,093	55,115	- 2,022	5.1	5.3	- 0.2
College: 1 to 3 years.....	29,160	25,835	+ 3,325	6.8	6.1	0.8
4 years or more....	37,765	25,309	+ 12,456	11.5	7.7	3.8
<i>The West</i>						
Grade School: Less than 5 years...	7,722	1,743	+ 5,979	8.6	1.9	6.6
5 and 6 years.....	12,317	2,380	+ 9,937	14.7	2.8	11.8
7 and 8 years.....	72,866	13,941	+ 58,925	15.8	3.0	12.8
High School: 1 to 3 years.....	65,576	16,186	+ 49,390	12.2	3.0	9.2
4 years.....	85,545	20,530	+ 65,015	12.2	2.9	9.3
College: 1 to 3 years.....	36,488	11,356	+ 25,132	14.0	4.3	9.6
4 years or more....	26,311	12,782	+ 13,529	15.1	7.3	7.8

grants. This regular relationship is found for both males and females and in urban, rural-nonfarm, and rural-farm areas.

The relationship is affected by distance spanned, however. The proportion migrating within a State is about the same among persons completing various numbers of elementary grades and indeed is slightly higher for those completing fewer than 5. That the more educated also migrate further is illustrated by the fact that almost one-third of migrating college graduates went to a

noncontiguous State as opposed to less than one-seventh of migrants completing up through 4 grades of elementary school.

Interregional movements were quite selective with respect to educational attainment. (See Table 2.) In the literature of regionalism, concern has been expressed about the indirect burden of migration upon the South's educational system. During the period 1935 to 1940 it is true that the South had a net loss of young adults 25 to 34 years old with only grade or high school educa-

tion. She had a net gain of persons in these ages with some college training. Practically all of the education concerned must have been received prior to migration. Even allowing for the greater cost of a year of college over a year of elementary or high school and the greater average cost of a given year of schooling in the North and West, the South undoubtedly had spent more money on the education of her out-migrants than the other regions spent on the in-migrants she received from them.

The Northeastern and North Central States gained persons with less than seven years of schooling and had net losses of the better educated. The West had net gains at all educational levels, the median years of school completed by the net migrants (12.4 years) exceeding that for the same age group in her 1940 population (11.9 years). Even as a percentage of the corresponding 1940 population in the region, some of the movements were impressively large. For example, the South's in-migrant college graduates represented 11.5 per cent, of her college graduates, the out-migrant college graduates from the North Central States represented 12.5 per cent of their college graduates, and the West's net migrants at the seventh and eighth grade levels represented 12.8 per cent. (The 1935 population would be a better base for these percentages.)

No comparable data on migration status by education are available for the war years.

MIGRATION AND EMPLOYMENT STATUS

Statistics on the employment status of persons classified by migration status in 1940 must be interpreted with considerable caution. We do not know the employment status of the migrant just before he moved. The bare facts are that of nonmigrant males in the labor force, 84.9 per cent were employed in 1940 whereas of migrant males 87.2 per cent were employed.⁹ The percentage employed was higher for each type of migration according to distance spanned than non-

migrants. The percentage on emergency work was particularly low for interstate migrants, undoubtedly partly because of residence qualifications for obtaining work relief. Migrants, except those between noncontiguous States also had a smaller proportion seeking work than nonmigrants. The situation was similar among females in the labor force.

It seems rather unlikely that migrants in the labor force compared as favorably with nonmigrants at the time they moved or that they would have been as well off had they stayed in their home counties. (One uncontrolled factor though is the greater concentration of the migrants in the age groups where the incidence of unemployment was lower.) There may be some evidence here of the role of migration in adjusting labor supply to job opportunities, but one could probably reach more definite conclusions by studying the characteristics of streams between specific areas and comparing them with the characteristics of the nonmigrant population in the areas of origin and destination. During the war, of course, there was very little unemployment. Data for the period 1941 to 1945 show that the per cent employed among migrants was about the same as among nonmigrants.

MIGRATION AND OCCUPATION

Migration status for the period 1935 to 1940 varied extensively with major occupational group among employed males 14 years old and over.¹⁰ Part of this variation is in accord with the previously discussed variation of migration status with education. The highest proportion of migrants (over one-quarter) was found among professional and semi-professional workers. Next, however, came service workers, both domestic and other, with between 17 and 18 per cent migrants. Least mobile were farm operators with about 8 per cent, just about half the percentage among farm laborers. The percentage for clerks and salesmen (15.8) was only a fraction above that for proprietors, managers, and officials. Other percentages were 13.4 for craftsmen and foremen, 12.6

⁹ U. S. Bureau of the Census. Sixteenth Decennial Census. Population. Internal Migration, 1935 to 1940: Economic characteristics of migrants. Washington, Government Printing Office, 1946.

¹⁰ *Ibid.*

for operatives, and 12.0 for nonfarm laborers. Migrating professional workers spanned much greater distances on the average than migrating farmers.

It is true that occupation may have changed between 1935 and 1940, especially at the time of migration. If the data were in terms of occupation in 1935, the proportion of migrants would probably be higher for farm operators and farm laborers.

It was shown above that the South had a net gain between 1935 and 1940 in persons 25 to 34 years old with some college training. This region also had a net gain of about 14,000 employed male professional and semi-professional workers, or 3.2 per cent of all such workers living there in 1940. The net gain was 9 per cent or more for Delaware, Virginia, Florida, and Maryland (for Virginia and Maryland, probably in considerable part around Washington, D.C.). Gross interstate shifts of male professional and semi-professional workers were very great relative to the net ones. For instance, 18.6 per cent of such workers in New Hampshire in 1940 were in-migrants. Out-migrants during the quinquennium constituted 18.1 per cent of this base, yielding a net gain of only 0.6 per cent.

PERSISTENCE OF MIGRATORY PATTERNS

Because of lack of data, relatively little has been said so far about wartime migration. An effort will be made in conclusion, however, to examine the consistency of wartime and prewar movements with respect to direction and volume. It is possible to make comparisons between wartime and prewar migration in terms of net gains and losses by States, since estimates of net migration, by States, based chiefly on ration book data are available for the period 1940 to 1943. Recently published estimates, developed from the ration book estimates, for the period 1940 to 1945 are also available.¹¹ In preparing these estimates, the patterns of gains and losses between 1940 and 1943 were preserved, but the rate of net change through migration was decreased somewhat.

¹¹ U. S. Bureau of the Census. Population—Special Reports. Estimated population of the United States, by States. Series P-46, No. 3. Feb. 12, 1946.

The net interstate shift (which represents the sum of the net gains through migration for the States that gained, or the sum of the net losses for the States that lost) was about 1,367,000 between 1935 and 1940; the net interstate shift in the civilian population between 1940 and 1945 is estimated at almost 4,000,000. Clearly, there was a much greater reshuffling of population among the States during the war period.

The problem here is to compare patterns of net gains and losses during the war period with those of earlier periods. The comparisons were made in terms of coefficients of correlation. This method permitted the computation of summarizing measures of similarity between the war period and various other periods in the past. Seven periods were selected for study. These are:

1. Net migration for 1940 to 1945, 1935 to 1940, 1930 to 1940, and 1920 to 1930.¹²
2. State birth-residence indexes (i.e. net gain or loss through interstate migration of the native population), for 1940, 1930, and 1920.¹³ The object in using State-of-birth data was to obtain historical patterns of net migration with which to compare the wartime pattern.

Net migration by States for the period 1940 to 1945 was correlated with the other six variables. A comparison of the coefficients was expected to yield some basis for generalization with respect to whether wartime trends were divergent and what might seem reasonable as prospects for the future.

Net migration, by States, for the period 1940 to 1945 has a positive correlation with net migration for each of the other three specific periods for which data were available. The coefficients are as follows:

with 1935 to 194092
with 1930 to 194079
with 1920 to 193081

There is a closer similarity between the

¹² Shryock, Henry S., Jr. "Internal migration and the war" in *Journal of the American Statistical Association*. 38 (122): 20. March, 1943. Estimates for 1920 to 1930 adapted from Thornthwaite.

¹³ U. S. Bureau of the Census. Sixteenth Decennial Census. Population. State of birth of the native population, Table 14.

pattern of net migration during the war and the pattern for the five years preceding 1940 than between the wartime pattern and either of the other periods. It may at first appear strange that the war period does not correlate more strongly with the 1930 to 1940 period than with the earlier decade. This circumstance is perhaps explained by the fact that the 1930 to 1940 decade as a whole embraces the effects of both the depression and the Dust Bowl crisis whereas the 1935 to 1940 period contains the beginnings of recovery and avoids the full force of the more depressed part of the decade. In fact, estimates of net migration between 1930 and 1935, obtained by subtraction, had a coefficient of correlation with the war period of only .49. These figures suggest that the similarity of the 1940 to 1945 period to the 1935 to 1940 and the 1920 to 1930 periods can be tentatively explained in terms of their common relative "normality" in patterns of migration. The high correlation between the 1940 to 1945 and 1935 to 1940 periods indicates that the earlier of the two periods could have been used to predict the pattern of the 1940 to 1945 period with a fair degree of success.

To obtain more historical perspective, the coefficients of correlation of the war period with the three birth-residence indexes were compared. The coefficients are as follows:

with birth-residence index, 194087
with birth-residence index, 193079
with birth-residence index, 192071

These relations give a measure of the resemblance of the wartime pattern to patterns that reach further into the past. The resemblance is closest to the 1940 birth-residence index, but not so close as to the net migration for 1935 to 1940 period. Since State of birth data refer only to the native population, some difference is rather to be expected.

One point should be mentioned in connection with the absolute sizes of the coefficients of correlation. The net migration values for California were so large that they exerted a powerful influence on the results obtained. To test the potency of this influence, coefficients omitting California were computed.

This procedure cut the values of the coefficients from a high of .92 to a high of .75 and from a low of .71 to a low of .45. All coefficients remained positive and significant, however. The rank order was only slightly changed from that obtained originally and the observations made above still hold. Since California did in fact exert a powerful influence on migration patterns in this country throughout all the periods covered, this State ought not to be disregarded in any analysis of whatever kind. It is proposed, therefore, that the original coefficients be accepted as computed with a mental allowance for the over-generous contribution of California to the attractiveness of the results.¹⁴

The coefficient of correlation (.92) between the 1935 to 1940 and 1940 to 1945 periods based on number of net migrants as the highest of those obtained, is the critical one for this analysis. A variety of tests indicated that the correlation was statistically highly significant. Its 95-per cent confidence limits range from .87 to .96. Examination of the scatter plot elicited the guess that the plotted point for California was in close alignment with the points for the rest of the States, though lying at some distance from the cluster. The regression equation was therefore evaluated, first excluding, then including, California. The slope of the regression line is 2.17 excluding California and 2.29 including California. From the regression equation excluding California, the 1940 to 1945 value for that State was predicted with an error of less than 7 per cent.

With this reassurance that values for California were not distorting the relations observed, at least so far as these two five-year periods were concerned, the coefficient of determination was computed including

¹⁴ It would have been possible, of course, to use percentages instead of absolute numbers of net migrants for the correlations—possibly the ratio of net migration to the population at the end of the period. This would have prevented California from having undue weight, but it would have given undue weight to some of the other States, such as Arizona and Nevada, which had very small numbers of net migrants and very small total populations.

that State. The value of r^2 is .85, or, in other words, the variation for 1935 to 1940 "explains" 85 per cent of the variation for 1940 to 1945.

From correlation analysis, then, we have established the resemblance of the 1940 to 1945 pattern of net migration to patterns in earlier periods, especially to the five-year period immediately preceding. A presumption is created that the next five years will bear a close resemblance to the five years just past.¹⁵ The greater volume of the interstate shift between 1940 and 1945 is, in general, an intensification of past shifts, probably partly the result of the release of "backed-up" movement accumulated during the thirties as well as of accelerated economic activity attendant to the war. From the slope of the regression equation, it is observed that a change of one unit in X implied or predicted a change about $2\frac{1}{4}$ times as large in Y.

It is not suggested that an accurate prediction for the period 1945 to 1950 can be made on the basis of our knowledge of the association between the 1935-to-1940 and 1940-to-1945 periods, either as to pattern or as to volume. The weight of the findings, however, are on the side of a continuation of the 1940 to 1945 pattern. Besides, it is doubtful that postwar reverse migration will be sufficient to alter the basic pattern very much, since the wartime excess is partly a post-depression release and since, having followed prior trends, it may be regarded as partly a normal movement.

There is evidence in the State of birth data that after World War I, migration increased in volume. The net interstate shift of the native population was 1,650,000 for

the decade 1910 to 1920 and 3,290,000 for the decade 1920 to 1930. There are few signs of reverse migration. To be sure, the postwar period began before 1920 and that war was so short that decennial data may be misleading. However, the decade of the 1920's was one of postwar prosperity and the brisk relocation of the population is no doubt one result of these good times.

Of course, it can be argued that World War II was also itself a period of economic prosperity and that the population has already shifted. Again, our findings are that shifting is a continuous process which follows a rather persistent pattern. If we are to have a period of prosperity following World War II, the odds are that the patterns observed between 1940 and 1945 will be largely preserved in the next 5 or 10 years. Aside from the factors already mentioned, areas of economic opportunity do not as a rule relocate rapidly enough to alter the pattern in a few years' time.

If, on the other hand, we lapse into economic doldrums and do not pull out by 1950, there is likely to be a great deal of return migration in the interim and the net interstate shift for the decade 1945 to 1950 will be smaller than for the period 1940 to 1945—a sort of 1930 to 1940 picture in reverse.

As in many types of time series, it may be that migration in one period is usually best predicted from migration in the immediately preceding period. The secular trend is probably following an orderly development rather than changing by abrupt jerks. There are important cyclical modifications of the secular trend, however. For instance, the pattern of migration by States for the period 1935 to 1940 would not have been best predicted from the pattern for 1930 to 1935. Probably the major part of the prediction of cyclical fluctuations in internal migration is the prediction of cyclical changes in economic activity.

¹⁵ This judgment is consistent with the conclusion reached by A. J. Jaffe and Seymour L. Wolfbein. "Internal migration and full employment in the U.S." *Journal of the American Statistical Association*. 40 (231): 351-363. Sept., 1945.

EFFECT OF THE WAR ON THE SEX RATIO AT BIRTH*

R. J. MYERS

Social Security Administration

A WIDESPREAD popular belief is held that during wartime there are considerably more males born than females. During peace years there are slightly more males, the sex ratio at birth customarily being about 1055, or in other words for every thousand girl babies there are 1055 boys. This paper will examine the latest available data for the war years 1942-45; a complete analysis involving the post-war years cannot be made for perhaps 5 years.

It has been shown that during the two decades between the World Wars the sex ratio at birth in this country showed no significant variance when the important factor of varying race proportion in the registration areas was considered.¹ For white persons the sex ratio at birth during the peacetime years averaged about 1060 (i.e., 1060 males per 1000 females), while that for nonwhites was approximately 1030.

During the First World War and shortly thereafter the sex ratio at birth in certain European countries rose significantly from a statistical viewpoint but not from a layman's viewpoint although in the United States the limited available data indicated no significant variation. Thus, for Germany the ratio was 1064 for 1915-18, 1075 for 1919-20, and 1070 for 1921-23, with the corresponding figures for the United Kingdom being 1047, 1055, and 1048, respectively.² In other words, the middle period showed about a 1 per cent increase over the two end ones, which were closely in line with previous and subsequent experience. The man on the street would not view this rise as significant, but for such a stable element as sex ratio at birth professional statisticians found significance present. However,

confounding any analysis, it was discovered that there was also an increase in the sex ratio at birth for certain European non-belligerents for this same period.

Official United States census data are now available for the three complete calendar years 1942-44 during which this country was in the Second World War. In addition, final data are available as to the total births by sex for 1945 (but not detailed data as to age of mother, race, and birth order). An analysis may be made as to the trend of the sex ratio at birth during this period as contrasted with the prewar quinquennium when the registration area was complete (even though there was not complete registration within each State).

For the period 1937-41 the sex ratio at birth for all races combined was 1053.8, with a range of only from a low of 1052.2 for 1938 to a high of 1054.8 for 1940. For 1942-45 the sex ratio was 1056.2 (being 1058.4, 1055.4, 1055.8, and 1055.4, respectively for each of the four years). Thus the sex ratio at birth was about $\frac{1}{4}$ per cent greater during the war years than during the previous five years, an amount which would hardly cause the layman to be impressed.

Considered from another angle, during the 4 wartime years there were reported 5,791,212 male births and 11,274,112 total births.³ If the sex ratio of 1937-41 had persisted through 1942-45 there would have been 5,784,700 male births so that there was actually an excess of only about 6,500 boys in the war years. The excess number of boys was thus only 0.1 percent, certainly no great increase in masculinity in births from a layman's viewpoint. However, the statistician could calculate the significance by obtaining the standard error based on the formula $\sigma = \sqrt{Npq}$ where N is total births

* Manuscript received November 25, 1946.

¹ "A Note on the Variance of Sex Ratios," R. J. Myers, *Human Biology*, September, 1943.

² "Statistical Study of the Sex Ratio at Birth," W. T. Russell, *Journal of Hygiene*, July, 1936.

³ Total births, allowing for under-reporting, have been estimated at 11,998,000 (from various official Census Bureau estimates).

in 1942-45, p is the probability of a birth being male (based on 1937-41 data), and q is the corresponding probability for female births. The standard error is computed to be 1,680 so that the difference between actual and "expected" of 6,500 is 3.9 times the standard error, which indicates that there is a significant difference, or in other words that significantly more boys were born during the war than would be expected on the basis of peacetime experience.

A further examination of the data indicates that a sizable amount of the excess male births in 1942-45 can be accounted for by two factors—the larger proportion of white births and the larger proportion of low order births. White persons have a higher sex ratio at birth than colored as indicated previously, and there is distinctly a higher sex ratio at birth for first births than for later ones. Based on data for 1942-44, the ratio for first births was 1064, for second births, 1059 for third births 1051, for fourth births 1050, and for fifth and higher order births 1039; part of this decreasing trend, but by no means all, is due to the higher proportion of colored mothers for the higher orders of birth.

For 1942-44 colored births constituted 11.74 per cent of the total births as against 12.38 per cent in 1937-41. Correspondingly, the recorded data by order of birth for all races combined are as follows:

Order of Birth	1937-41	1942-44
First	37.7%	37.7%
Second	24.4	26.5
Third	13.4	14.3
Fourth	8.0	7.8
Fifth and higher	16.5	13.7
Total	100.0	100.0

The proportion of first births is the same for the two periods because of an unusually low proportion for 1944 (namely, 34.4 per cent), probably due to the prolonged absence from this country of young single men and married men without children who made up the bulk of our armed forces.

Other factors affecting the sex ratio at birth include age of parents, nationality, economic status, and legitimacy.⁴ These gen-

erally have a smaller influence than race and birth order. Age of mother appears to have a significant role, but this is in larger part due to the correlation between birth order and age of mother. Thus from Table 1 for white births in 1942-44 it may be seen that for all orders of birth combined the ratio shows a decreasing trend with advancing age of mother. But when the analysis is "standardized" by considering each order of birth separately (as in the middle section of the Table), no trend by age of mother is apparent. Moreover, with "standardization" by age of mother, there is a rather clearly defined decreasing trend for higher orders of birth.

In order to measure the effect of the different race and birth order distribution for 1942-44 as compared with the previous 5 years, it is desirable to adjust the actual percentage of male births in 1937-41 on the basis of the distribution by race and birth order in the war years. However, this must be done indirectly by following the reverse procedure (namely, obtaining the change in the overall percentage of male births in 1942-44 on the basis of the distribution by race and birth order in the prewar years) because data on sex of births by race and order are available only since 1941.

Table 2 shows the actual 1942-44 births and the actual proportion of male births by race and birth order (excluded from the figures shown in this table are those births where the order was not stated). Also shown is the redistribution of these total births on the basis of the distribution of the 1937-41 births, and the number of these births that would be male on the basis of the 1942-44 proportions for each race and birth order group separately. Totalling up these estimated male births on the basis of the 1937-41 distribution yields an over-all male proportion of 51.359 per cent as compared with the actual proportion of 51.374 per cent, or a decrease of .015 per cent resulting from a distribution of the 1942-44 births by race and

⁴For an excellent discussion of the various elements involved, see "An Inquiry into Methods of Studying the Sex Ratio at Birth for the United States during War and Post War Years," Rachel M. Jess, *Human Biology*, September, 1943.

TABLE 1. SEX RATIO OF WHITE BIRTHS IN 1942-44 BY AGE OF MOTHER AND BIRTH ORDER

Age of Mother	Total Births	Order of Birth				
		First	Second	Third	Fourth	Fifth & Higher
Sex Ratio of Births						
Under 15	1030	1024	*	*	*	*
15-19	1068	1069	1067	1055	975	*
20-24	1062	1068	1058	1053	1049	1049
25-29	1063	1066	1065	1068	1053	1046
30-34	1054	1060	1056	1052	1058	1046
35-39	1054	1064	1053	1060	1056	1048
40-44	1043	1075	1075	1033	1067	1033
45 & over	1038	*	*	*	*	1048
All Ages	1060	1067	1060	1058	1054	1045
As Percent of Proportion for All Ages						
Under 15	97.2%	96.0%	*	*	*	*
15-19	100.8	100.2	100.7%	99.7%	92.5%	*
20-24	100.2	100.1	99.8	99.5	99.5	100.4%
25-29	100.3	99.9	100.5	100.9	99.9	100.1
30-34	99.4	99.3	99.6	99.4	100.4	100.1
35-39	99.4	99.7	99.3	100.2	100.2	100.3
40-44	98.4	100.7	101.4	97.6	101.2	98.9
45 & over	97.9	*	*	*	*	*
Total	100.0	100.0	100.0	100.0	100.0	100.0
As Percent of Proportion for all Orders of Birth						
Under 15	100.0%	99.4%	*	*	*	*
15-19	100.0	100.1	99.9%	98.8%	91.3%	*
20-24	100.0	100.6	99.6	99.2	98.8	98.8%
25-29	100.0	100.3	100.2	100.5	99.1	98.4
30-34	100.0	100.6	100.2	99.8	100.4	99.2
35-39	100.0	100.9	99.9	100.6	100.2	99.4
40-44	100.0	103.1	103.1	99.0	102.3	99.0
45 & over	100.0	*	*	*	*	101.0
All Ages	100.0	100.7	100.0	99.8	99.4	98.6

* Not computed because less than 1000 total births.

order in accordance with the pattern of 1937-41 rather than 1942-44.

Conversely, it may safely be concluded that if the data were available to adjust the actual over-all proportion of male births in 1937-41 (namely, 51.310 per cent) on the basis of the distribution by race and birth order in the first 3 war years, there would be an increase of .015 per cent, yielding an adjusted proportion of 51.325 per cent. On this basis the "expected" male births in 1942-45 would be 5,786,400, or an excess of actual over "expected" of about 4,800 which is 2.9 times the standard error and is therefore barely within the range of normal

statistical fluctuation with significance not definitely indicated.

Canada, like the United States, has shown no appreciable upward trend is the sex ratio at birth during the war years. Thus, for 1940-43 the ratio was 1058 as against 1055 in the previous 4 years. However, in 1942 an unusually high ratio of 1067 was reported, but this was counterbalanced by a low ratio for 1940 and only average-size ones for 1941 and 1943.

In summary, there is no evidence of any appreciable increase in masculinity of births in the United States and Canada during the Second World War, especially when ac-

TABLE 2. EFFECT OF RACE AND BIRTH ORDER ON PROPORTION OF MALE BIRTHS IN 1942-44

Order of Birth	Actual 1942-44 Data		Births on Basis of 1937-41 Distribution	
	Total Births	Proportion of Male Births	Total ^a	Male ^b
White				
First.....	2,784,916	51.623%	2,758,676	1,424,111
Second.....	1,959,889	51.468	1,786,491	919,471
Third.....	1,026,083	51.418	951,284	489,131
Fourth.....	536,487	51.313	548,328	281,364
Fifth and higher.....	852,363	51.103	1,053,652	538,448
Total ^c	7,159,738	51.466	7,098,431	3,652,525 ^d
Non-White				
First.....	273,475	50.816%	302,495	153,716
Second.....	2,052	50.896	197,187	100,360
Third.....	136,077	50.484	138,778	70,061
Fourth.....	98,967	50.613	101,665	51,456
Fifth and higher.....	262,038	50.544	284,391	143,743
Total ^c	963,209	50.690	1,024,516	519,336 ^d
All Races				
Total ^c	8,122,947	51.374 ^e	8,122,947	4,171,861 ^d
		51.359 ^f		

^a Total reported births of 1942-44 redistributed by race and order of birth according to distribution of 1937-41 births.

^b Total 1942-44 births allocated on basis of 1937-41 distribution multiplied by actual 1942-44 proportion of male births.

^c Excludes births where birth order was not stated.

^d Obtained by totalling figures by birth order.

^e Actual figures based on 1942-44 data.

^f Derived figure from 1942-44 births allocated on basis of 1937-41 data.

count is taken of the higher proportion of white births and of lower order births. From a statistical viewpoint there was a possibly significant higher sex ratio at birth during the war years than during peacetime, but the absolute amount of the increase was so small relatively as to be considered negligible from the layman's viewpoint. Even in the First World War the relatively large increase

in certain European countries, as measured by statistical tools, was not sizable enough to support the common belief that there is a great increase in masculinity of births during wartime.⁵

⁵ For detailed discussion of the European experience during wars, see "Are More Males Born in Wartime?", Constantine Panunzio, *Milbank Memorial Fund Quarterly*, July, 1943.

A CHECK ON UNDERENUMERATION IN THE 1940 CENSUS*

DANIEL O. PRICE†
University of North Carolina

UNDERENUMERATION of underregistration is the bane of every census and of every Bureau of Vital Statistics the world over. Possibly the main difference in this connection between a good census set-up and a poor one is that the first will attempt some estimate of the proportion underenumerated while the latter will go blithely on in ignorance of the fact that it is missing some of the population. Two checks are possible: a sample check or a mass enumeration which would be both compulsory and subject to penalties. Spot samples, to anticipate our data by a glance forward at Tables 1 and 2, can vary so much from area to area that a mass census is indicated as the only valid check on enumeration.

For the first time in history we are provided by the First Selective Service Registration with a check that fulfills the requirements, for it was compulsory on all males between 21 and 35 in 1940 and was backed by adequate penalties. Other difficulties were present, however, and should not be underestimated as the following account will show. After estimating the amount of underenumeration for all classes and then for Negroes separately an attempt is made to evaluate the factors associated with the Census errors.

The First Selective Service Registration was on October 16, 1940, just six and one half months after the date of the Sixteenth Census. This registration stated that:

Every male person (other than persons excepted by section 5(a) of the aforesaid act¹) who is a citizen of the United States or an alien residing in the United States and who, on the registration date fixed herein, has attained the twenty-first anniversary of the day of his birth and has not attained the thirty-sixth anniversary

of the day of his birth, is required to present himself for and submit to registration.²

Section 11 of the Selective Training and Service Act of 1940 authorizes "imprisonment for not more than five years or a fine of not more than \$10,000, or both such fine and imprisonment" for those who knowingly failed to register. Offsetting to a certain extent the fear of penalty was the desire on the part of some to escape registration and military service. On the other hand there was the force of patriotism and public opinion which probably more than counterbalanced this desire to escape military service.

The registration figures used are those for June 30, 1941.³ Figures are available for the number registered on October 20, 1940, but these were quick, preliminary totals and do not include the late registrants. The June 30, 1941, figure is approximately 250,000 higher than the first quick count taken on October 20, 1940, just five days after the initial registration. This increase is not due to the registration of persons coming of draft age during this period because the first draft covered just those persons 21-35 inclusive on October 16, 1940.

In order to get a Census figure comparable with the Selective Service Registration it is necessary to "age" the 1940 Census figures by six and one half months, i.e., from April 1 to October 16, 1940. In order to do this, three factors are taken into consideration—the number aged 20 on April 1 who became 21 by October 16, the number aged 35 on April 1 who became 36 prior to October 16 and thus were not subject to the First Registration, and the number in this age group, 21-35, dying between the two dates

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† Prepared while holding a fellowship from the Social Science Research Council.

¹ This section excluded persons already in the armed forces.

² First Registration Day Proclamation by the President of the United States of America. Found in *Selective Service in Peacetime*. Government Printing Office, 1942, p. 387.

³ *Selective Service in Peacetime*. Government Printing Office, 1942, p. 392.

under consideration. Since those becoming 21 between April 1 and October 16, 1940, were born between April 1 and October 16, 1919, the actual per cent distribution of births by months in 1919⁴ was applied to those aged 20 on April 1, 1940, to make the first correction. The distribution of births by months is unavailable for 1904, so in order to make allowance for those becoming 36 during this six and a half month period, the average monthly distribution of births over a five year period, 1929-1933, was used.⁵ The death rate of this age group during the six and a half months of 1940 under consideration was used in correcting for number of deaths.⁶

The Selective Service Registration gives the number of Negroes registered⁷ and thus it is possible to make a separate check on completeness of enumeration of Negroes. The Negro population of April 1, 1940, was aged in a manner similar to that described above for the entire population. Inasmuch as Negro ages are not given by single years of age it was necessary to assume that for any given year of age the ratio of Negroes to Nonwhites is the same as the ratio of Negroes to Nonwhites for the five year period including that single year of age.

By aging the Census figures in this manner for these two groups we arrive at state totals that are comparable with the Selective Service Registration figures. The results are shown in Tables 1 and 2. Table 1 shows the aged Census figure for each state for all classes, the total Selective Service Registration in each state, and the difference between these two figures as an absolute number and as a percentage of the Census figure. Negative numbers in the last two columns indicate that the Census figure is larger than the Selective Service Registration. The states are arranged in order of the per cent discrepancy between the Selective Service Registration and the

Census figure. Table 2 shows similar figures for the Negro population for those states which contain appreciable numbers of Negroes. The data indicate an underenumeration of about 2.8 per cent in this total age-sex group. For Negroes the number missed is almost 15 per cent of the number enumerated.

However, certain considerations must be kept in mind. These tables do not take into account those persons in the armed forces and thus excluded from the Selective Service Registration. It is practically impossible to get an accurate estimate of this number and allocate them by states. However, in 1940 the Census counts 77,000 of the Army, Navy, Marines, and Coast Guard as stationed in the United States.⁸ We do not have the age distribution of this group but since the figure does not include officers it seems reasonable that two thirds or about 50,000 might be in the 21-35 age group. No doubt a negligible portion are Negroes. Combining this figure with the number registered by the Selective Service shown in Table 1 we get as our estimate of the number of males 21-35 in the United States October 16, 1940, the figure 16,615,000—a figure 3.1 per cent higher than the Census count of the same age group. Thus our data lead to the conclusion that the Census enumerates approximately 97 per cent of the male population in this age group. Here we have one of the first major checks ever made on the completeness of the Census enumeration and find that taking the population as a whole the Census seems to do a fairly reliable job.

This figure of 97 per cent coverage is based, of course, on the assumption that the Selective Service Registration was 100 per cent complete. We know that this is unlikely though a close approximation was probably attained.

It is when we come to consider the Negro population that the Census enumeration seems least reliable. Here the number missed amounts to 14.88 per cent of the number enumerated. This amounts to enumerating

⁴ *Birth, Stillbirth, and Infant Mortality Statistics, 1910*. General Table 1, p. 75.

⁵ *Birth, Stillbirth, and Infant Mortality Statistics, 1933*. Table E, p. 7.

⁶ *Vital Statistics of the United States, 1940*. General Table 8, p. 173, and General Table 10, p. 179.

⁷ Selective Service in Peacetime, p. 392.

⁸ *Comparative Occupational Statistics for the United States 1870-1940*. Table 14, p. 165.

TABLE 1. COMPARISON OF CENSUS COUNT AND SELECTIVE SERVICE
REGISTRATION OF MALES AGED 21-35 INCLUSIVE

Area	1940 Census Count "Aged" to October 16, 1940 ^a	First Selective Service Registration ^b	Difference Between Census Count and Selective Service Registration	
			Number	Percent of Census Count
United States.....	16,112,171	16,565,037	452,866	2.81
District of Columbia.....	99,053	114,863	15,810	16.0
Nevada.....	15,866	17,469	1,603	10.1
Florida.....	239,700	250,264	16,564	6.9
Louisiana.....	293,293	312,999	19,706	6.7
California.....	904,427	957,325	52,898	5.8
South Carolina.....	230,365	242,181	11,816	5.1
Michigan.....	650,633	681,374	30,741	4.7
Idaho.....	68,047	71,217	3,170	4.7
Tennessee.....	354,864	370,932	16,068	4.5
West Virginia.....	230,046	240,379	10,333	4.5
New Jersey.....	525,992	547,213	21,221	4.0
Illinois.....	980,673	1,017,613	36,940	3.8
Connecticut.....	215,638	223,440	7,802	3.6
New Mexico.....	64,118	66,403	2,285	3.6
Arkansas.....	228,847	236,955	8,108	3.5
Delaware.....	34,042	35,215	1,173	3.4
Alabama.....	339,466	350,949	11,483	3.4
New York.....	1,669,405	1,723,162	53,757	3.2
Indiana.....	404,977	417,861	12,884	3.2
Ohio.....	829,231	854,605	25,374	3.1
Pennsylvania.....	1,214,618	1,249,859	35,241	2.9
Missouri.....	433,674	446,219	12,545	2.9
Texas.....	809,886	831,104	21,218	2.6
Georgia.....	391,151	401,079	9,928	2.5
Mississippi.....	258,857	265,135	6,278	2.4
Maryland.....	237,083	242,465	5,382	2.3
Virginia.....	341,212	348,228	7,016	2.1
Montana.....	72,321	73,770	1,449	2.0
Maine.....	92,383	93,925	1,542	1.7
New Hampshire.....	55,053	55,919	866	1.6
North Carolina.....	448,445	455,182	6,737	1.5
Colorado.....	134,047	135,908	1,861	1.4
Kentucky.....	330,422	334,416	3,994	1.2
Massachusetts.....	498,910	504,282	5,372	1.1
Oregon.....	134,204	135,298	1,094	0.8
Rhode Island.....	85,070	85,719	649	0.8
Wisconsin.....	371,485	371,071	- 414	-0.1
Arizona.....	63,636	63,478	- 158	-0.2
Iowa.....	288,574	287,258	- 1,316	-0.5
Minnesota.....	334,651	331,802	- 2,849	-0.8
Utah.....	66,453	65,760	- 693	-1.0
Washington.....	220,185	216,584	- 3,601	-1.6

^a See text for method of aging Census count.

^b *Selective Service in Peacetime*. Government Printing Office, 1942. P. 420.

TABLE 1—(Continued)

Area	1940 Census Count "Aged" to October 16, 1940 ^a	First Selective Service Registration ^a	Difference Between Census Count and Selective Service Registration	
			Number	Percent of Census Count
North Dakota.....	78,006	76,572	-1,434	-1.8
Nebraska.....	148,061	145,127	-2,934	-2.0
South Dakota.....	73,191	71,627	-1,564	-2.1
Oklahoma.....	275,853	269,191	-6,662	-2.4
Vermont.....	40,977	39,963	-1,014	-2.5
Kansas.....	201,757	196,476	-5,281	-2.6
Wyoming.....	34,607	33,201	-1,406	-4.1

TABLE 2. COMPARISON OF 1940 CENSUS COUNT AND FIRST SELECTIVE SERVICE
REGISTRATION OF NEGRO MALES AGED 21-35 INCLUSIVE

Area	1940 Census Count "Aged" to October 16, 1940 ^a	First Selective Service Registration ^b	Difference Between Census Count and Selective Service Registration	
			Number	Percent of Census Count
United States.....	1,537,203	1,765,917	228,714	14.88
District of Columbia.....	27,277	38,288	11,011	40.3
Illinois.....	45,746	61,245	15,499	33.8
New Jersey.....	27,239	34,161	6,922	25.4
New York.....	73,416	91,988	18,572	25.3
California.....	17,233	21,361	4,128	24.0
Missouri.....	27,982	34,375	6,393	22.8
Delaware.....	4,643	5,686	1,043	22.4
Pennsylvania.....	53,804	65,177	11,373	21.1
Ohio.....	39,819	47,385	7,566	19.0
Florida.....	71,825	84,137	12,312	17.1
Maryland.....	39,083	44,948	5,865	15.0
Louisiana.....	98,979	112,894	13,915	14.0
Texas.....	112,150	128,540	16,390	14.6
Virginia.....	79,761	90,437	10,676	13.4
South Carolina.....	91,110	102,824	11,714	12.9
Kentucky.....	24,553	27,495	2,942	12.0
Alabama.....	110,047	123,072	13,025	11.8
Georgia.....	128,979	143,920	14,941	11.6
Tennessee.....	62,262	68,777	6,515	10.5
Arkansas.....	55,269	60,818	5,549	10.0
West Virginia.....	14,269	15,665	1,396	9.8
North Carolina.....	120,708	129,501	8,793	7.3
Oklahoma.....	19,051	20,153	1,102	5.8
Mississippi.....	123,743	130,472	6,729	5.4

^a See text for method of aging Census count.^b *Selective Service in Peacetime*. Government Printing Office, 1942. P. 392.

only about 87 per cent of the total, or 13 per cent underenumeration.⁹ A probable factor in this discrepancy is the difficulty of enumerating Negro ages, that is the digital preference for even ages and ages divisible by five,¹⁰ which is especially marked in the Nonwhite group. This digital preference tends to balance itself out in a series of ages and upsets mainly the extreme ages of the group under consideration. In this particular age grouping, 21-35 inclusive, it seems that the clustering effect at ages 20 and 21 would more than offset any upward bias at age 35. Thus it seems likely that the digital preference for ages does not have a great deal of effect on this particular age grouping. If this is the case, then the figures in Table 2 give us reason to believe that the Census enumerates only about 87 per cent of the male Negro population in this age group.

An examination of the variations from state to state in enumeration of Negroes, shown in Table 2, suggests that much of the variation is probably due to migration between the time of the Census and the time of the Selective Service Registration. Thus the areas showing the highest percentage of Negroes missed by the Census are those areas to which there is normally the heaviest Negro migration. Out-migration reduces the discrepancies in the areas from which movement takes place and makes the discrepancies appear larger in the areas to which the migrants go. These same variations could be explained on the assumption that Negroes in urban areas are less completely enumerated than Negroes in rural areas, though migration seems to be the more easily justified

explanation. However, both causes probably combine to make up the large variations shown in Table 2. It is impossible to separate these two causes and their combined effect is so large as to obscure the possible effects of other factors.

Looking again at the figures in Table 1, the question arises, can we generalize this 97 per cent enumeration to the population not under Selective Service? We have seen that it does not apply to Negroes, but would it apply to total males of all ages, or even the total population? The answer to this question depends, of course, on the cause of the underenumeration. It might be a constant factor present at all ages and in both sexes, or it might be associated with characteristics which vary with the age-sex group under consideration. It seems reasonable to assume that at least a portion of the underenumeration is associated with population characteristics of this latter sort, e.g. mobility and lack of permanent domicile, characteristics of young males. In this case underenumeration cannot be generalized to the entire population and might even be maximum in this age-sex group since it includes those most mobile and least likely to have permanent residences or strong home ties that would bring them into the Census enumeration. In order to investigate some of these possibilities correlation coefficients were computed between the per cent discrepancies shown in Table 1 and several measurable population characteristics.

One of the first things likely to affect the completeness of the Census enumeration is the density of the population. Here the correlation of .11 is in no way statistically significant, nor is the correlation of .14 between these discrepancies and the per cent of the population urban. We can say on this point that if density of population or urbanism affects the completeness of the Census enumeration we can not demonstrate this effect in the total population even though the data on Negroes would indicate such a relationship. As for the effects of education, we get a correlation of -.25 between the discrepancies of Table 1 and the median school year achieved. This correlation is on the borderline of statistical significance but does

⁹ This apparent change of figures from 14.88 to 13 per cent is due to the fact that 14.88 per cent of the Census count is only 13 per cent of the total population because the Census count does not include the total population. For example, if the total population consisted of 9 people and only 6 were counted, the 3 missed amount to one half the number counted but only one third the total population.

¹⁰ Myers, Robert J., "Errors and Bias in the Reporting of Ages in Census Data," *Transactions, Actuarial Society of America*. Vol. 47, Part 2, No. 104, pp. 395-475. October-November, 1940.

lend credence to the idea that the lower the educational level of a state the less complete the Census count tends to be. Communications facilities might be expected to have some bearing on this question, but the correlation of $-.13$ between the discrepancies and telephones per 1000 population gives no evidence that these two things are related.

One of the highest correlations was found just where we would expect it after examining the variations in enumeration of Negroes, that is, between the discrepancies and the net male migration 1935-40.¹¹ This correlation is $.50$. A correlation of $.59$ was obtained between the discrepancies and the per cent population change from July 1, 1940, to July 1, 1941, as estimated by the Census.¹² This indicates that the variations in the discrepancies shown in Table 1 are made up of two parts—one part due to actual underenumeration and the other due to the migration that took place between the time of the Census and that of the Selective Service Registration. Since there is no simple and direct way of separating these two factors making up the discrepancies we are forced to conclude that we cannot obtain accurate estimates of the Census underenumeration by states.

In an attempt to determine if unattached males are more likely than others to be missed by the Census, the correlation was computed between the discrepancies and the per cent of males 14-34 that are single. A negative correlation of $-.40$ indicates the

reverse, i.e. that the more single males there are in a state the more complete the Census enumeration tends to be. There seems to be no readily apparent reason why such an association should exist.

Conclusions: This comparison of the Selective Service Registration and the Census shows that apparently the 1940 Census underenumerated all males 21-35 by about three per cent, and underenumerated Negro males 21-35 by about 13 per cent. A study of the variations between Selective Service and Census figures on a state basis gave no clue to the factors associated with underenumeration except migration between the time of the Census and the Selective Service Registration.

The three per cent underenumeration of all males in this age group is not large enough to be of great concern, but if Negro males are underenumerated by 13 per cent, there are many questions we might well ask ourselves. What are the true death rates of Negro males? What should a life table for Negro males really look like? How many Negroes are there in our population? This amount of underenumeration also leaves little support for those who, on the basis of Census discrepancies in numbers, attempt to point out that so many Negroes "pass" as whites between Censuses. These results also make it seem that the advocates of "white supremacy" in Mississippi might have been celebrating prematurely when the 1940 Census showed that Mississippi for the first time had more whites than Negroes. If Negroes in Mississippi were underenumerated by the 5.4 per cent shown in Table 2 then actually in 1940 Negroes still predominated in that state.

¹¹ *Sixteenth Census of the United States 1940, Population, Internal Migration 1935-40. Color and Sex of Migrants.* Table 8, p. 18.

¹² Census Release, Series P-44, Number 11.

ECONOMIC DIFFERENCES AMONG RURAL CENTERS*

VINCENT HEATH WHITNEY

Brown University

SICANT attention has been given differences in the economic and social organization of variant types of rural centers. Descriptions of agricultural villages, which serve primarily as service centers for surrounding open-country hinterlands, have been made to do for all places. The fallacy of such generalization is that it fails to recognize the differences in structure and role which occur in rural centers of different size and functional type.

certain illustrative economic differences among the 126 villages and hamlets which constitute the total of rural centers in a ten-county subregion of northcentral North Carolina.¹ The distribution of these centers by size and type is indicated in Table 1.

Locational factors. The location of a center is of considerable importance in its effect upon village structure. It is a factor probably secondary only to the economic function of the village. The agricultural centers

TABLE 1. SIZE-TYPES OF VILLAGES AND HAMLETS, NORTH CAROLINA SUBREGION, 1940

Size class	Number of Centers			
	Agricultural	Industrial	Suburban	Total
I—25-99 population.....	41	0	3	44
II—100-249 population.....	27	5	5	37
III—250-749 population.....	11	11	4	26
IV—750-1249 population.....	5	3	2	10
V—1250-2499 population.....	5	3	1	9
All size-classes combined.....	89	22	15	126

Source: Field schedules of the author.

This is not to argue that in both structure and function villages and hamlets do not possess a group similarity in which individual deviations are overshadowed. Group differences, of course, receive increased emphasis as we approach extremes of size and type; yet all villages have characteristics in common which may be observed and measured. Such characterization and measurement, however, must take into account variations both in degree and in kind. For among agricultural, industrial, suburban, lumbering, mining, political and resort villages, there are marked variations in structural and functional organization. These variations appear to be associated particularly with locational and occupational factors.

The discussion here will be limited to

of the Subregion are, as a group, significantly farther from urban places of 10,000 or more population² and considerably farther from

¹The ten counties are Alamance, Caswell, Chatham, Durham, Granville, Guilford, Orange, Person, Rockingham, and Wake. A description of the area and of the methodology of selection will be found in my unpublished manuscript, *The Pattern of Village Life* (Chapel Hill: The Institute for Research in Social Science, 1944), Ch. 3. See also, Vincent Heath Whitney, "The Rural-Nonfarm Population: Patterns of Growth in a Piedmont Area," *Social Forces*: 24-1 (October 1945), pp. 81-89; and Charles S. Johnson, et al., *A Statistical Atlas of Southern Counties* (Chapel Hill: The University of North Carolina Press, 1941). The field work was done in 1940-41.

²For a hypothetical universe this has been statistically demonstrated in *The Pattern of Village Life*, op. cit., Ch. 10 and Appendix B. For example, a comparison of the mean distance of agricultural and industrial villages of 1250 to 2499 population from the closest urban center of 10,000 or more popula-

* Manuscript received August 14, 1946.

all urban places than are industrial or suburban centers. Obviously, people within their community areas will give them greater trade support than will the people of a suburban center, many of whom go daily to a nearby city to work and who depend upon local stores primarily for items forgotten in the city or representing the same article on sale at approximately the same price, for example, a loaf of bread. To a lesser extent, the same is true for industrial centers, whose relative proximity to urban places has been noted. Since this is true, we should expect to find a greater number of stores in agricultural centers than in suburban and industrial centers of corresponding size. And that this is the actual situation is apparent from Table 2.

Numbers of business units. As we might expect, within each category an increase in population is accompanied by the ability to support a greater average number of stores. There is, however, considerable difference in the number of business units found in centers of the same size class but of different functional types. In size classes I through IV the agricultural centers without exception support a larger average number of business units than do the suburban centers; and the suburban centers are in the same relationship to the industrial centers. In size class V, however, the industrial villages support a larger average number of business units than are found in the single suburban village of this size. Not only is this pattern uniform, but as the population classes increase, the differential between the agricultural villages on the one hand and the suburban and industrial villages on the other becomes striking. The availability of economic goods in centers of different types thus shows considerable variation.

It may be noted too that an increase in size in rural centers is accompanied not merely by an increase in number of stores but as well by greater specialization by type of

store.³ For example, in agricultural hamlets the general store is the most common type. In fact, except for a few grocery stores,

TABLE 2. MEAN NUMBER OF BUSINESS UNITS SUPPORTED, BY SIZE-TYPES OF CENTERS, THE SUBREGION, 1940†

Centers by Size-type Classification*	Mean Number of Business Units	
	Excluding Filling Stations	Including Filling Stations
Agricultural—I.....	1.4	2.0
Suburban—I.....	0.7	1.3
Industrial—I.....	—	—
Agricultural—II.....	4.2	5.2
Suburban—II.....	2.8	3.8
Industrial—II.....	1.6	1.6
Agricultural—III.....	14.5	17.0
Suburban—III.....	6.5	7.5
Industrial—III.....	4.8	6.4
Agricultural—IV.....	46.8	54.2
Suburban—IV.....	15.5	19.5
Industrial—IV.....	13.0	16.7
Agricultural—V.....	63.2	69.8
Suburban—V.....	11.0**	15.0**
Industrial—V.....	26.0	29.7
Agricultural—IV and V.....	55.0	62.0
Suburban—IV and V.....	14.0	18.0
Industrial—IV and V.....	19.5	23.2

Source: Field schedules of the author.

† Originally reproduced in V. H. Whitney, "The Estimation of Population for Unincorporated Places," *American Sociological Review*: 11-1 (February 1946), p. 99.

* See Table 1.

** One village only.

garages, and fertilizer warehouses, it is the only type of trade unit present. In the small agricultural villages (size III) a greater diversity of store types appears. Most of these centers have one or more general stores, but their dominant position in the center has

tion, shows the observed difference between the means to be significant since, where $t = 3.721$ with 14 degrees of freedom, $.01 > P > .001$. In the sub-region a complete enumeration rather than a sampling situation was involved.

³ This has been noted in other studies. Compare, for example, Edmund deS. Brunner and J. H. Kolb, *Rural Social Trends* (New York and London: McGraw-Hill Book Co., 1933), pp. 145-146.

declined.⁴ Found in greatest number are groceries and markets, cafés, drug stores, garages, and feed, seed, and fertilizer stores. Barber shops and beauty shops approach universality, and furniture and electric appliance stores are common. All of these stores appear in greater number in the large vil-

lage which the village is capable of attracting. There are definite differences in both the kinds of stores and of merchandise available in the centers of different size. And one advantage which the large village has over the small village or hamlet is that the potential customer who wants a specialized

TABLE 3. PERCENTAGE OF STORES WITH CUSTOMERS FROM SPECIFIED POINTS, THE SUBREGION, BY COMBINED SIZE-TYPES, 1940

Centers by Size-types	Three-fourths or More of Customers from*				
	Center Only	Center and Nearby Farms	Farms in 10 Mile Area	Farms Over 10 Miles	Nearby Urban
Hamlets (I-II)					
Agricultural**	6.2	37.5	47.3	3.6	0
Industrial	100.0	0	0	0	0
Suburban***	83.3	0	8.3	0	0
Small villages (III)					
Agricultural	0	22.4	68.3	9.2	0
Industrial	90.0	0	3.3	6.7	0
Suburban	100.0	0	0	0	0
Large villages (IV-V)					
Agricultural	21.6	0	24.4	52.7	1.3
Industrial	83.3	0	12.5	0	4.2
Suburban	100.0	0	0	0	0

Source: Field schedules of the author.

* In order to keep the basic unit stores rather than answers, the replies of those merchants who indicated roughly equal division of their customers among two or more sources have been treated as fractions of one in figuring percentages.

** Also: transient, 3.9; sawmills throughout county, 1.0; city over 10 miles away, 0.5.

*** Also: nearby village, 8.3.

lages (sizes IV and V) and in addition still more specialized shops, such as those of jewelers, florists, and photographers, occur. The department store as distinguished from the general store first appears in this class of villages, and the chain Five-and-Ten is also found. A few general stores, catering to farm trade almost exclusively, remain, but they have either become far more elegant than their counterparts in the smaller places or are back-street catch-alls hoping to benefit from some of the overflow of open-country

article (such as a dress shirt) and a non-specialized article (such as a gallon of molasses) tends to make both purchases at the center where the more highly specialized article is available.

Trade factors. Similarly, structural peculiarities of size-types of villages are associated with their principal occupations. The result is again a marked difference in the number and kind of services available to the people of the center. Further, such structural differences are reflected in differences in the number and kind of associations of the people of a given size-type of center with other rural and urban groups.⁵ This becomes readily

⁴ Brunner and Lorge noted in 1936 that the village general store had continued a general decline initiated between 1910 and 1920. Edmund deS. Brunner and Irving Lorge, *Rural Trends in Depression Years* (New York: Columbia University Press, 1937), pp. 102-103.

⁵ Cf. *The Pattern of Village Life*, op. cit., Ch. 8, and especially, Table 31, p. 217. For example, among centers of 1250-2499 population, agricultural vil-

apparent when we consider the difference in basic function in the three types of centers. The agricultural village is a farm service center. The industrial village is a producing center. And the suburban village is an urban overflow or satellite center.

The agricultural centers of the Subregion furnish no exception to the generally accepted statement that the existence of agricultural villages depends primarily upon open-coun-

sponding size and 83.3 and 100 per cent of the suburban stores look to the center itself to supply the majority of their customers. Among the large agricultural villages, the greater population base and the consequent increase in small markets, drug stores, and other trade units which appeal for the support of the villagers, account for an increase in the percentage of stores depending mainly on the center for support to 21.6. The group

TABLE 4. REASONS ASSIGNED BY MERCHANTS FOR CUSTOMER PATRONAGE, THE SUBREGION, BY COMBINED SIZE-TYPES, 1940

Centers by Size-Types	Reasons for Purchases by Number of Times Mentioned								
	Low Price	High Quality	Personal Friend- ship	Accessi- bility	Wide Stock	Credit Obtain- able	Old Busi- ness	Fair Treat- ment	Other
Hamlets (I-II)									
Agricultural.....	0	0	4	19	7	6	3	1	5
Industrial.....	0	0	0	2	0	0	0	0	1
Suburban.....	0	1	2	3	0	1	0	1	0
Small villages (III)									
Agricultural.....	1	0	3	6	2	1	1	2	4
Industrial.....	2	1	3	5	1	3	0	0	2
Suburban.....	1	1	1	2	0	0	0	0	1
Large villages (IV-V)									
Agricultural.....	13	2	4	4	11	1	5	0	9
Industrial.....	1	1	0	3	0	4	1	1	1
Suburban.....	1	0	1	0	1	0	1	0	0

Source: Field schedules of the author.

try trade. Table 3 indicates clearly that the trade units of such centers exist largely on the patronage of customers from beyond the villages themselves.

From the point of view of their customer areas the outstanding difference between the types of centers is the difference in the dependence of their stores upon the center itself for trade support. Only 6.2 per cent of the stores in agricultural hamlets carry on three-fourths or more of their business with residents of their own center; and in the small agricultural villages the percentage is zero. On the other hand, 100 and 90 per cent of the stores in the industrial centers of corre-

differential is maintained, however, for 83.3 per cent of the stores in comparable industrial villages and 100 per cent of those in such suburban villages must survive on local support only. Here is tangible evidence of a difference in the character of the centers, for no question of size is involved.

Here too is support for the apparent existence of a lack of sympathy between rural industrial and rural farm peoples, which is reflected in concrete ways.⁸ One of these is by the lack of farm patronage of industrial-center stores. A similar lack in suburban villages is due to a different reason. The trade resources of the suburban center are no more adequate than those of agricultural centers

lages averaged 8.2 organized associational groups exclusive of churches; industrial villages, 3.5; and suburban villages 4.3.

⁸ Cf. Edmund deS. Brunner, *Industrial Village Churches* (New York: Institute of Social and Religious Research, 1930), pp. 21-22.

closer to the majority of farmers and open-country non-farm dwellers. When members of these groups have driven a distance of some miles, they naturally are interested in the specialized trade units, the larger variety, the better-quality goods which they can expect to find in the city. The suburban villages too have made little effort to build local open-country trade through promoting organizations for farmers and villagers or through making the center in any way especially attractive to trade from other areas. It should be noted too that in the Sub-region at least those industrial centers which have any appreciable amount of open-country patronage are exclusively those which were once agricultural centers, into which mills have moved in recent decades. Trade patterns have been built which continue in part from habit.

Among the agricultural centers the areas of open-country patronage increase in outer distance from the center as population grows.⁷ Among the agricultural hamlets only 3.6 per cent of the stores claim farm trade from a distance of over ten miles. Among the small agricultural villages 9.2 per cent of the stores indicate considerable patronage from such a distance. But among large agricultural villages the comparable percentage is 52.7. A close dependence upon open-country trade areas for growth, and indeed survival, is thus seen to be characteristic of the agricultural centers.⁸

The large villages are able to provide a greater proportion of their trade from their own inhabitants. Whereas 6.2 per cent of the stores in agricultural hamlets depended upon the center for three-fourths or more of their business, and these were stores generally too poorly stocked or located to attract open-country purchasers, and none of the stores in the small agricultural villages received its chief support from villagers, in the large agricultural villages 21.6 per cent of the trade units did only an incidental business with persons outside the center. Here is an-

other way in which the large village differs in its trade function from the smaller centers.

It is of interest that 20 per cent of all merchants interviewed could give no reason for customer patronage of their particular stores. The reasons assigned by the remaining 80 per cent appear in Table 4. These represent, of course, opinion attitudes and, as such, must be regarded as less reliable than behavior attitudes.

The reasons given show considerable variety, and undoubtedly many of them represent guesswork. Among the miscellaneous reasons totaled in column nine, merchants mentioned better terms, the attraction of advertised "bargains," force of habit, and the cleanliness and general appearance of the store. Several merchants considered that their villages were good farm markets, and that from that point on, the choice of a store was largely accidental. Others suspected that customers were coming to them because they owed money elsewhere and so were forced to shift stores.

Of the reasons mentioned more frequently, it is notable that merchants in the hamlets and small villages place considerable emphasis on the factor of accessibility. Minor emphasis goes to personal friendship between proprietor and customer and the opportunity to obtain credit. Comparatively little emphasis is placed on low price, high quality, or wide variety, except that the latter was mentioned by seven merchants who operate general stores in agricultural hamlets. The trends are not well-defined in the large industrial and suburban villages though the reasons most prominent in the industrial villages are accessibility and the obtaining of foods on credit. In the large agricultural villages, however, major emphasis shifts to low price and wide stock. Thus, the reasons cited are indicative of the changing character of merchandising in the different sizes and types of centers and of the fundamentally variant attraction of trade units in the separate categories.⁹

⁷ Topography and the location of competing centers influence specific local patterns.

⁸ For a general summary of changes in trade areas, see Edmund deS. Brunner and J. H. Kolb, *op. cit.*, pp. 153-161.

⁹ Cf. James F. Page, *Relation of Town and Country Interests in Garfield County, Oklahoma* (Stillwater: Oklahoma Agricultural Experiment Station Bulletin 194, 1930).

Table 5 indicates sharp differences between groups of centers in terms of the amount of business conducted on a cash and on a credit basis. The percentage of stores in agricultural centers doing business primarily on a cash basis is considerably higher than in the comparable industrial and suburban centers. Similarly, in the hamlets and large villages, the proportion of stores operating

TABLE 5. METHOD OF PAYMENT OF STORE RETAIL SALES, THE SUBREGION, BY COMBINED SIZE-TYPES, 1940

Centers by Size-types	Percentage of Stores with Sales Primarily:		
	Cash	Credit	Divided*
Hamlets (I-II)			
Agricultural.....	31.5	37.0	31.5
Industrial.....	0.0	100.0	0.0
Suburban.....	0.0	66.7	33.3
Small villages (III)			
Agricultural.....	26.1	39.1	34.8
Industrial.....	13.3	86.7	0.0
Suburban.....	16.7	16.7	66.6
Large villages (IV-V)			
Agricultural.....	54.1	13.5	32.4
Industrial.....	16.7	75.0	8.3
Suburban.....	25.0	25.0	50.0

Source: Field schedules of the author.

* Roughly, half on a cash and half on a credit basis.

primarily a credit business is smaller for the agricultural centers, even with "crop credit," than for either of the other types. In small agricultural villages, the percentage is higher than in the suburban villages but considerably below that of the industrial villages. The high percentage of credit business associated with the industrial group merits particular notice. Of the stores visited, 100 per cent in the industrial hamlets, 86.7 per cent in the small industrial villages, and 75 per cent in the large industrial villages operate primarily on a credit basis.

Further, the percentage of stores operating on a cash basis tends to increase and the percentage operating on a credit basis to decrease as the population of the center becomes larger.¹⁰ In the bigger places depend-

ence upon the trade of a particular customer or upon that of a small group of customers is not as heavy as in the small centers, which have a narrower drawing power. The merchant is not as often faced with the unhappy choice of doing business on a credit basis or of doing no business at all. This same situation is true of the agricultural as compared with the industrial and suburban centers with their smaller or non-existent open-country trade hinterland. In the smaller places too relationships between merchant and customer are more apt to be those between friends, or at least, between neighbors. The personal nature of such contacts makes the majority of small-center merchants regard a large credit business as inevitable even though they would prefer operation on a cash basis. Such a preference is understandable in view of the relatively heavy losses many of them have suffered where short-time credit has turned out to be permanent. Many a storekeeper agreed with the proprietor of a general store in a small agricultural village who complained that not over 10 per cent of the people in town would trade with him any more. He had let the other 90 per cent have credit when they couldn't pay cash. And now, since they owed him, they were ashamed to come in even when they had cash for current business.

As one struggling grocery in an industrial village pathetically summarized the cash-credit dilemma in a home-made sign:

I will feed your pigs
And mind your baby.
But I won't give credit
And I don't mean maybe.

This was a 100 per cent credit business, the proprietor wryly told me.

Industrial factors. One final economic difference between industrial centers on the one hand and agricultural and suburban centers on the other is found in their manufacturing establishments. Table 6 shows the mean number of small, medium, and large plants in the several size-types of center in the Subregion.¹¹

appears to be reasonable probability that it exists elsewhere.

¹¹ Small manufacturing establishments employ

¹⁰ Cf. *Ibid.*, p. 16. Page found the same general tendency in Garfield County, Oklahoma, and there

The large plants, as might be expected, are found almost entirely in the industrial centers. The only ones not so located are in

TABLE 6. MEAN NUMBER OF INDUSTRIAL ESTABLISHMENTS PER CENTER, THE SUBREGION, BY COMBINED SIZE-TYPES, 1940

Centers by Size-types	Mean Number of Industrial Establishments		
	Small*	Medium*	Large*
Agricultural			
Hamlets.....	0.4	0.03	0.0
Small villages.....	0.8	0.1	0.0
Large villages.....	1.4	0.8	0.6
Industrial			
Hamlets.....	0.0	0.0	1.0**
Small villages.....	0.0	0.0	1.2***
Large villages.....	0.1	0.0	2.8***
Suburban			
Hamlets.....	0.3	0.0	0.0
Small villages.....	1.0	0.0	0.0
Large villages.....	0.0	0.0	0.0

Source: Field schedules of the author; and *Industrial Directory and Reference Book of the State of North Carolina* (Raleigh: Dep't of Conservation and Development and Dep't of Labor, 1938).

* See footnote 11.

** Mill closed in two centers at time of survey.

*** Mill closed in one center at time of survey.

the large agricultural villages. For the most part these are cotton, rayon, or hosiery mills. The small plants, chiefly planing mills, saw mills, cotton gins, grist mills, and the like, are closely associated in character with an agricultural economy and are found chiefly in the agricultural centers. A few are found in suburban hamlets and small villages, but for the most part the suburban centers are lacking in manufacturing establishments. No medium-size or large plants are found in any of the suburban centers, and no plants of any size in the large suburban villages. Industrial centers, for their part, have almost no auxil-

from 3 to 15 workers; medium from 16 to 100 workers; and large, over 100 workers. The data have been obtained, where possible, in the field. They have been checked and additional figures supplied by use of the *Industrial Directory and Reference Book of the State of North Carolina* (Raleigh: Dept. of Conservation and Development and Dept. of Labor, 1938).

iary manufacturing establishments. One or two large mills are universal, but medium or small plants in addition are not generally found.

The dominating position of employment in manufacturing in the industrial villages spells a different way of life from that of agricultural centers where commerce and farming dominate¹² and from that of suburban centers where commercial employment in a neighboring city is most common. Just as the agricultural village exists primarily as a commercial and social service center, the industrial village is in major function a production center whose existence is chiefly dependent upon a single major industry. In varying degrees that industry dominates the structure and organization of the village. When full operation of the plant is maintained, the village flourishes; when industrial output lags, the life of the village does the same; and when such a plant closes, the village experiences a rapid decline unless it is near other centers of employment where full operation continues.

The industrial village has, of course, a trade and a social function, but it does not perform these for any large number of non-villagers. By and large, mill villagers and farmers are unsympathetic. Their daily worlds are separated by a wide gulf with the result that the stores, the schools, the churches, and the associations of the industrial villages fail to attract open-country people. Lack of such support is reflected in the smaller number of stores, schools, and organi-

¹² Several writers have stressed the predominance of manufacturing over trade in "villages" and the small numbers of villagers engaged in agriculture. See, for example, Newell Sims, *Elements of Rural Sociology* (New York: Thomas Y. Crowell Co., 1940), p. 113. In the Subregion commerce far exceeded manufacturing as a source of employment in agricultural villages, and of a total population of 45,427 some 15 per cent were actively engaged in farming. Of the latter, 49.6 per cent were in agricultural hamlets and 81.7 per cent in agricultural centers; 12.2 per cent in industrial centers; and 6.4 per cent in suburban centers. T. Lynn Smith found that in Louisiana "a considerable number" of rural-farm people were resident in villages. See *The Population of Louisiana: Its Composition and Changes* (University: Louisiana Agricultural Experiment Station Bulletin 293, 1937), p. 26.

zations which are found in centers of this type. Here is clear evidence of the effect of function upon structure.

If the agricultural village may be thought of as an open center and the industrial village as a closed center, the suburban village may perhaps be described as an attached center. The chief function of the suburban village is as a place of residence for an urban overflow of persons primarily engaged in commercial or industrial activities in a neighboring city. The structure of the suburban center is strongly influenced by this dependence upon the nearby urban place which is one of its outstanding characteristics.

Conclusion. In summation then, the data of this study clearly indicate significant differences among villages and hamlets of vary-

ing size and functional type which make blanket generalizations hazardous.¹³ Manifestly, there are many similarities which link villages of a given type and set them off from other population aggregates, both urban and rural. Yet it is equally apparent that the centers surveyed exhibit considerable diversity of form and function both when considered as a total number and as individual personalities. On a pragmatic level, such differences are closely associated with the ability of a center to offer sufficiently complete economic and social resources to afford its people a satisfying life experience.

¹³ Only economic data have been presented here, but political and social distinctions likewise exist. These are recorded in *The Pattern of Village Life*, *op. cit.*, Chapters 7 and 8.

INTENSITY AND A ZERO POINT FOR ATTITUDE ANALYSIS*

LOUIS GUTTMAN

Cornell University

EDWARD A. SUCHMAN

Social Science Research Council

I. THE NEED FOR A ZERO POINT

IN COMMON parlance, people are said to be happy or unhappy, to be intelligent or stupid, to have a favorable or unfavorable attitude toward something, to be for or against a certain political candidate. Such dichotomizations are made with respect to characteristics that are recognized not to consist simply of two categories; in common parlance, *degrees* of happiness, intelligence, or favorableness are considered to exist. The problem that we wish to consider in this paper is: Is there any sense to dividing people into two kinds—positive and negative—when the variable under consideration comprises many differences in degrees?

This problem occurs in much of attitude analysis and especially in public opinion analysis. Public opinion polls attempt to state, for example, how many people are "pro-Russia" and how many are "anti-Russia," how many are "pro-labor" and how many are "anti-labor," how many are "in favor of" certain governmental policies and how many are "against" these governmental policies, etc., etc. The problem of how to determine such dichotomizations is not in general susceptible to solution by the use of an external criterion like going to the polls and voting, or contributing money, or some consideration. Such external criteria are important and of great interest, but seem to afford only an indirect approach to the problem of defining a cutting point which is intrinsic to the attitude or opinion. It is conceivable that a person is "against" a certain candidate but will vote for him because he is even more "against" the opposing candidate, or he is "against" a given proposi-

* Presented to the Fortieth Annual Meeting of the American Sociological Society, Cleveland, March 3, 1946. A detailed discussion of the theory and method of content and intensity measurement will be published soon by the Social Science Research Council, part of a report based upon materials gathered by the Research Branch, Information and Education Division, War Department.

tion but will endorse it, if the only alternative is one he considers even worse; or he may be "for" a candidate but even more so "for" the opponent, or "for" a proposition but even more so "for" an alternative one. Furthermore, different external criteria will produce different cutting points. For example, should a man be considered "pro-labor" or "anti-labor," if he contributes money to a labor welfare fund, but still crosses a picket line?

It seems, then, that an intrinsically meaningful cutting point should be defined in a manner that will not depend on an external criterion. If such an internal dichotomization can be made, then in addition to solving some current methodological problems, it will open up new avenues of research in attitude and public opinion analysis which should increase the understanding of the depth or intensity of feeling with which opinions are held, and should also facilitate predictions. For example, of all people who are favorable to a candidate, how many will turn out to vote for him? Of all people who are well-adjusted now in their marriage, how many will become divorced in the future?

The need for an internal definition of a zero point is especially vital for public opinion polls. Research workers in this field are acutely aware of the problem of "bias" in the wording of questions, and thus far there has been no satisfactory solution to this problem. It is well-known that the wording of questions, the order of presentation of questions, the wording of the check-list of answers, or the use of free responses in answers, and a whole host of related things, can change the apparent opinion of the respondent. Furthermore, any single question asked on an issue is but one example of all possible questions that could be asked on that same issue, and the proportion saying "agree" or "yes" to these questions can range from zero to 100 per cent.¹ For example, a study of soldiers' attitudes toward the British asked, "Do you agree or disagree

with the following statements about the British? . . .

. . . The British are doing as good a job as possible of fighting the war, everything considered.

. . . The British always try to get other countries to do their fighting for them."

Response	First Statement	Second Statement
Agree	80	47
Disagree	17	48
Undecided	3	5
Total	100	100

We find 80 per cent of the soldiers *agreeing* to the first statement, but only 48 per cent *disagreeing* to the second statement. Which of these questions is an "unbiased" question with respect to the entire issue?

The problem of "bias," or differing results produced by differing questions, has been widely recognized by pollsters; indeed, the utility of most of their findings rests almost entirely on reaching a solution to this problem.

An approach to an internal definition of a zero point has been made by the Research Branch of the Information and Education Division of the War Department during the last year of the war. This approach begins with the idea of content scale analysis and, in fact, is a direct consequence or corollary of content scale analysis. It affords a determination of a zero point—if one exists for a given problem—that is completely objective. It does not depend upon the wording of any particular question and will divide the population into the same two parts on an issue regardless of the specific questions used. This means that the problem of "bias" of question wording or question selection is solved in an objective manner which does not depend on having "experts" judge the apparent "bias" of questions. One set of questions could be made up by a group of people with one particular axe to grind, and another set of questions with reference to the same issue could be made up by a group

¹ Compare the "tree" of public opinion in Hadley Cantril, ed., *Gauging Public Opinion*, Princeton University Press, 1944; pp. 26-27.

of people with the opposite axe to grind. No matter how these two sets of questions were worded, *provided only that the two sets pertain to the same single issue*, they will both yield the same proportion of people positive and negative on the issue.

At first, it may seem like an impossibility to obtain such an invariant answer. That it is not impossible, but in fact highly plausible that such an answer can be obtained, will become clear from considerations of what is involved in content scale analysis and in its corollary, the intensity function.

2. CONTENT SCALE ANALYSIS

Before we can speak meaningfully about people being divided into positive and negative, it must be ascertained whether or not it is at all meaningful to arrange people in rank order *along a single continuum* with respect to the particular area being studied. This means that the area must be tested to see if it is *scalable* for the given population of people. The theory of scale analysis has been presented before to this Society.² Briefly, the procedure is to consider a universe of content that is to be studied, like attitude towards the Army, attitude towards one's job, attitude towards Russia, etc., etc. The universe is considered to be indefinitely large—it consists of all possible questions that could be asked about one's attitude towards the topic being studied. This universe is sampled by making up a series of questions on the issue. At least ten to twelve items are ordinarily to be used in practice for the pre-testing in a preliminary study. The population of people is also defined, and a random sample of the population is chosen. The responses of the sample of people are then observed with respect to the

sample of questions. On the basis of these sample data, any of several equivalent techniques of scale analysis can be used to test the hypothesis that the universe of content is scalable for the population of individuals, e.g., has a single content variable for all respondents.

The Research Branch has been using scale analysis as a standard procedure for the past four years. Many areas of content have been found to be scalable, and very many have been found not to be scalable for given populations of people.

3. THE SAMPLING OF ITEMS

For our present purposes, it is important to recognize the distinction between the sampling of items or questions and the sampling of people. The sampling of people can be analyzed according to ordinary statistical procedures since the sampling can be done at random (with or without stratification) in the strict sense of the term. On the other hand, for the sampling of items or questions a completely different problem exists. The process of item selection or construction is not at all a random sampling process; it is a psychological process undergone by the research worker which, thus far, has little analytical theory behind it. It seems certain at present that ordinary sampling theory is inappropriate for the problem of item selection. Scale analysis affords an approach to an answer in that its results *do not depend on the particular sample of questions used*. No matter what sample of questions is used (provided only that the marginal frequencies are sufficiently different) the hypothesis can be tested that they are from a scalable universe of questions. The reason for this relative independence of the sample of questions is that the hypothesis of scalability is an extremely simple and highly restrictive one. If the entire universe of questions is scalable, then any sample of questions must prove to be scalable no matter how it is chosen. Hence, if a sample of questions is not scalable, this proves the universe is not scalable. If a sample of questions is scalable and is large

²Louis Guttman, "A Basis for Scaling Qualitative Data," *American Sociological Review*, 1944, 9: 139-150. A bibliography of published materials on scale and intensity analysis will be found in Louis Guttman, "Scale and Intensity Analysis for Attitude, Opinion, and Achievement," to appear in the *Proceedings of the Conference of Military Psychologists on Methodological Contributions to Psychology* to be published soon by the University of Maryland.

and diverse enough, then the inference is made that the universe is scalable.³

If a universe of content is scalable, it is meaningful to say that some people are higher than others on the universe and in fact to arrange the whole population in a rank order on the area from high to low. Furthermore, the ranking of the people on any sample of questions must be essentially that which would be obtained from the whole universe of questions. Any sample of items or questions from a scalable universe must rank the people in essentially the same order as any other sample of items. Thus, the rank order of individuals from high to low, or from more to less favorable, for one set of attitude or opinion questions will be the same as the rank order from a completely different set of questions on the same topic. This is the first invariance property that makes obtaining an invariant zero point possible. By obtaining another invariant ranking, on intensity, and relating these two invariant rankings, we obtain an invariant zero point.

4. INTENSITY ANALYSIS

If a given universe of content is not scalable, then it is not meaningful to order people from high to low, and in particular it is not meaningful to speak of people being positive or negative on the area. The lack of scalability indicates that more than one content variable is involved and no single ranking or division into pro and con can be made.

If an area is scalable, what intrinsic properties must a zero point have? There is meaning in the statement that one person is higher than another on the area because of the scale pattern. What should it mean to say that one person is positive and another person is negative? If a zero point were to be defined, in what respect are two people the same if one is a certain distance *above* the zero point and the other is the *same*

distance *below* the zero point? They are different in that the first is higher than the second in the content scale ordering, but they are the same in their distance from the zero point. What shall we name this second variable on which they are the same? The answer proposed here is to call this second variable the *intensity function*. If two people have the same intensity of feeling on an issue but differ in their position on the content scale, then they must be on opposite sides of the zero point.

If intensity of feeling goes up as one moves either to the right or to the left of the zero point, then intensity must be a U or J shaped function of the content scale order. The zero point can then be determined as that point on the content scale at which the intensity function reaches its minimum; that is, as that content point corresponding to the bottom of the U or J.

The problem is to measure the intensity function empirically. If this can be done, the zero point can be determined by plotting intensity of feeling against content score. This has been done by the Research Branch for many areas; in each scalable case, a U or J shaped curve emerged and a zero point was thereby approximately determined. The technique used for measuring the intensity function is far from perfect and better suggestions will undoubtedly be forthcoming in the future. However, crude as it is, the technique does work and gives fairly satisfactory results. Several examples will be presented here of the use of this technique.

5. ILLUSTRATIONS OF THE INTENSITY FUNCTION

A survey was conducted in June, 1945, among a cross-section of 1,730 enlisted men in the United States to determine the soldier's attitude toward the Women's Army Corps (the WAC). A series of six questions was asked as a sample of all possible questions in the universe of "attitude toward the WAC." One of these questions was, for example,

(a) "Suppose a girl friend of yours was con-

³ Like all inferences made about a population from a sample, this is, of course, not a completely certain inference.

sidering joining the WAC, would you advise her to join?

- Yes, I'm almost sure I would
- Yes, I think I would, but I'm not sure
- No, I probably would not
- No, I'm sure I would not
- Undecided"

In order to measure intensity of feeling, each of the questions was followed by another question [part (b)] asking,

(b) "How strongly do you feel about this?"

- Very strongly
- Fairly strongly
- Not so strongly
- Not at all strongly"

The parts (a)—the content—and the parts (b)—the intensity—were analyzed

produce the response of each individual to each item (within the margin of error provided by the coefficient of reproducibility of the scale), while in a quasi-scale this internal reproducibility is not possible. An important property that a scale and quasi-scale do have in common is that each provides an *invariant ordering* of people with respect to the sampling of questions, and this is all that we require for our present purpose. The rank order of respondents on a sample of items from a quasi-scale would be essentially the same rank order as that in the universe of all items that could have been used. Therefore, the ordering of the soldiers on content and on intensity is essentially invariant with respect to the particular sample of six questions used. Each of the six content

TABLE 1. ATTITUDE OF ENLISTED MEN TOWARD THE WAC

Intensity Rank	Content Rank							Total Frequency	Cumulative Percent
	(Neg.)						(Pos.)		
	0	1	2	3	4	5	6		
6 (High)	92	78	47	21	21	13	20	292	100
5	37	50	34	21	21	6	9	178	83
4	17	50	46	22	28	11	10	184	73
3	26	27	65	39	36	13	15	221	62
2	4	22	65	60	60	27	18	256	49
1	4	20	48	45	123	34	10	284	35
0 (Low)	3	12	61	59	146	30	4	315	18
Total Freq.	183	259	366	267	435	134	86	1730	
Cumulative Percent	11	25	47	62	87	95	100		
Midpoint of Content Percentiles	5	18	35.5	54	75	91	98		
Median of Intensity Percentiles	83	73	51	42	28	36	59		

separately for scalability. The six content questions proved to form a scale, and the intensity to form a quasi-scale. One essential difference between a scale and a quasi-scale is that from scale scores one can re-

and intensity questions were dichotomized to produce seven ranks. Each soldier received two scores: a content score ranging from 0 to 6 depending upon the number of questions upon which he held "positive" attitudes toward the WAC, and an intensity score ranging from 0 to 6 depending upon the number of answers about which he felt "strongly." Tabulating intensity score against content score for the 1,730 soldiers yielded the frequency distribution shown in Table 1.

* As will be reported in the forthcoming publication on the work of the Research Branch, intensity of feeling can be measured in many different ways. For example, the same results were obtained by asking after each attitude question, "How sure are you of your answer?" or "How hard was it for you to make this choice?"

If the pure intrinsic intensity function were being measured by the technique of using parts (b), then Table 1 should show intensity as a perfect U or J shaped function of content. Table 1 shows that this is not the case, that there is much error in the technique; but despite the considerable amount of error the essential shape of the intensity function is apparent. The italicized frequency in each column of Table 1 corresponds to the interval in which the median intensity lies for each content interval. A proper graphic presentation of the curve of medians is to express the data, not in the crude ranks observed, but in estimates of what the rank on the whole attitude universe would be if indefinitely many questions had been asked. The percentile metric is a way of doing this. The cumulative frequencies for the row (content) and column (intensity) marginals of Table 1 appear at the bottom and to the right of the table respectively. The last row of Table 1 indicates the estimated median percentile of each column. In Figure 1, the percentile metric is used for both content and intensity so that the people are considered to be arranged from zero to 100 per cent according to each of the two. Each plotted point corresponds to the midpoint of the interval on content and to the column median on intensity.

The curve in Figure 1 is essentially invariant. If a different sample of questions than these particular six had been used, essentially the same curve would result if plotted by the percentile technique.⁵

In particular, *the region in which the zero point lies is invariant*. It is not possible to ascertain exactly where the zero point is, but clearly it should be between 54 and 91, the midpoints of the intervals surround-

ing the lowest point of the plotted curve. We can therefore say that, according to this sample of soldiers, 54 per cent of the Army had an unfavorable attitude toward the WAC; 9 per cent had a favorable attitude toward the WAC; and the remaining 37 per cent were in between. As more and more questions are added to the scale, the size of the zero-range will become more narrow and the exact location of the zero-point can be determined with greater accuracy. On the basis of the present sample of six ques-

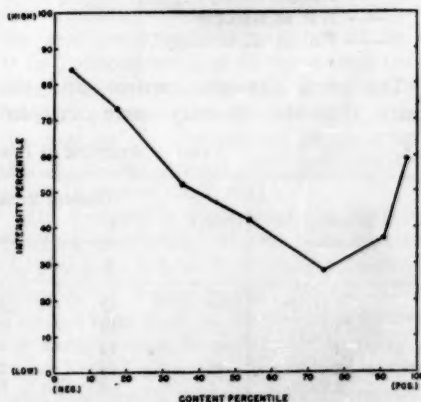


FIGURE 1. Attitude of the Enlisted Men Toward the WAC.

tions, the zero-point is approximately at 75 per cent.

The fact that the curve in Figure 1 is suspended in mid-air and does not reach the bottom percentile of intensity is due to the presence of error in the technique for ascertaining intensity. The column medians are being plotted; since there is error, the medians are away from where they would be if there were no error. The essential shape of the intensity curve that would be obtained if there were no error seems rather apparent from Figure 1, and it seems safe to assume that the pure curve would actually touch bottom in a region within the zero interval obtained from the observed curve.

A technique for obtaining a *single point approximation* for the zero point is to use

⁵ Several examples of similar curves and zero-points obtained from different samples of questions from the same attitude universe will be presented in the forthcoming publication on the work of the Research Branch. Despite the fact that these different samples of questions had widely different marginal distributions, the percentage of respondents to the right and left of the zero-point remained constant.

the median content percentile of the people lowest on intensity. In the perfect curve, the lowest person on intensity would be at the zero point on content. Since we have error present in practice, we can take an average of the content positions of the people lowest on intensity as an approximation to

of how the plan was being carried out in practice. Let us consider this second area first. Six questions were asked as a sample of the universe of content. As in the previous example, each question was actually in two parts; the first part concerned the content, and the second part elicited intensity of

TABLE 2. OPINION OF ENLISTED MEN ON THE ADMINISTRATION OF THE ARMY SCORE CARD PLAN

Intensity Rank	Content Rank							Total Frequency	Cumulative Percent
	(Neg.)						(Pos.)		
	0	1	2	3	4	5	6		
9 (High)	65	50	42	22	30	33	81	323	100
8	44	66	70	40	45	52	57	374	90
7	39	55	84	60	59	63	44	404	78
6	16	67	98	85	84	85	25	460	65
5	17	42	93	114	112	91	28	497	51
4	12	34	130	128	157	157	11	629	36
3	10	21	64	82	70	19	1	267	16
2	—	8	46	46	28	9	1	138	8
1	4	8	19	28	13	6	1	79	4
0 (Low)	4	18	23	15	7	4	—	71	2
Total Freq.	211	369	669	620	605	519	249	3242	
Cumulative Percent	7	18	39	58	76	92	100		
Midpoint of Content Percentiles	3	12	28	48.5	67	84	96		
Median of Intensity Percentiles	79	63	45	38	40	47	81		

the ideal zero point. As small a percentage as possible should be used in order to avoid distortion, but the sample number of people included must be large enough to be reliable. If we consider only the bottom row in Table 1, the median content percentile of the 315 men therein is, by interpolation, 66 per cent, and we can use this as an estimate of the true zero point. This estimate does fall in the zero interval just previously obtained from the U curve.

The shape of the intensity function can vary considerably from problem to problem. An interesting example is afforded by a study of soldiers' opinions of the Army's demobilization score card plan. Two areas were to be studied. One was opinion of the *idea* of the score card, and the other was opinion

feeling. One of the questions was, for example,

- (a) "In general, do you think the Army is trying its best to carry out the Army score card plan as it should be carried out?"
 — Yes, it is trying its best
 — It is trying some, but not hard enough
 — It is hardly trying at all
- (b) How strongly do you feel about this?
 — Not at all strongly
 — Not so strongly
 — Fairly strongly
 — Very strongly

Each part (a) of the remaining five questions asked opinion about how the plan was being carried out; each part (b) was identical with the above for all the questions.

The joint distribution of content and intensity ranks for the attitude of soldiers on how the score card plan was being carried out is given in Table 2, and the approximate intensity function is plotted in Figure 2.

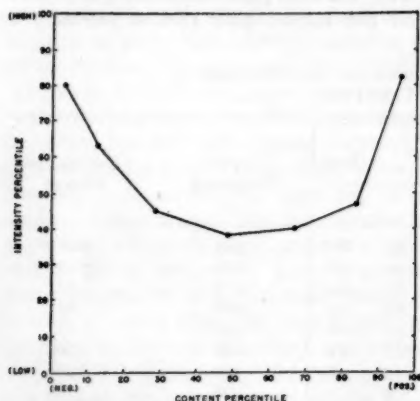


FIGURE 2. Opinion of Enlisted Men on the Administration of the Army Score Card System.

The expected U curve of intensity on content results with the low point of the curve, the zero-point, falling between percentile 28 and 67 of the content scale. There is indicated a split among the soldiers in their attitude toward the way in which the score card plan was being administered, with about half the soldiers having a favorable attitude and half having an unfavorable attitude. In general the curve has a rather flat bottom indicating a wide zero-range, or area of indifference. The shape of this curve is quite different from the curve on attitude toward the WAC.

The other aspect of the problem, the soldier's attitude toward the idea of the score card plan, produced an altogether differently shaped intensity curve. Seven questions, containing the content in part (a) and the intensity in part (b) were asked as a sample of all questions that could have been asked in this area.

The joint distribution of content and intensity ranks for soldiers' attitudes toward the idea of the score card plan is given in Table 3, and the approximate intensity

function is plotted in Figure 3. The zero point seems to be indefinitely far to the left! This indicates that there was little or no unfavorable opinion about the idea of the score card—practically everybody was favorable. Such a conclusion could not be ascertained by looking at the marginal frequencies of the individual questions asked in this area. For example, one of the questions was, "In general, what do you think of the Army score card plan (the point system)?" The responses were as follows:

23%	It is very good
49	It is fairly good
16	It is not so good
7	It is not good at all
5	Undecided
<hr/>	
100%	

It is important to notice that the technique of single point estimation of the zero point

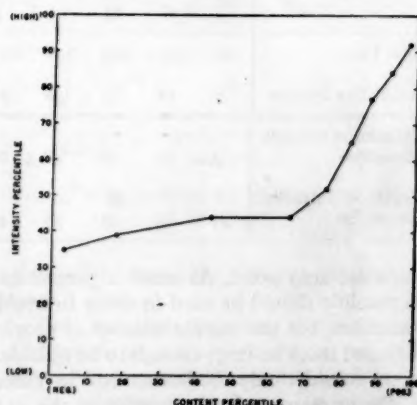


FIGURE 3. Attitude of Enlisted Men Toward the Idea of the Army Score Card Plan.

by using the content median of the lowest intensity group breaks down in this instance. Using the lowest intensity technique is not safe if the intensity function is too asymmetric.

Illustrations of other shapes of curves are presented in Figure 4, but further discussion will be reserved for the forthcoming publication of the Research Branch which

will summarize the findings of its four years of work in surveying soldier attitudes and opinions. Briefly, we can say that flat-bottom U shaped curves indicate that there is not much difference between pros and cons; except for those relatively few people who are very extreme on either side, most of the

strongly" to part (b) when they were undecided on part (a), they would answer to the effect that by "very strongly" they meant that the problem was very important, or else that they were very sure that they were undecided, or some such thing. Thus, many of the responses were out of context

TABLE 3. ATTITUDE OF ENLISTED MEN TOWARD THE IDEA OF THE ARMY SCORE CARD PLAN

Intensity Rank	Content Rank									Total Frequency	Cumulative Percent
	(Neg.)								(Pos.)		
	0	1	2	3	4	5	6	7	8		
8 (High)	26	58	75	26	17	30	46	54	104	436	100
7	15	62	60	27	16	28	34	45	12	299	87
6	18	46	73	32	19	30	34	19	8	279	77
5	13	54	102	32	36	31	10	10	4	292	69
4	19	55	113	67	38	28	18	5	—	343	60
3	20	91	144	84	42	23	11	3	2	420	49
2	15	80	157	66	29	22	7	3	3	382	36
1	32	119	177	70	27	8	6	2	—	441	24
0 (Low)	69	134	80	37	15	10	3	2	—	350	11
Total Freq.	227	699	981	441	239	210	169	143	133	3242	
Cumulative Percent	7	29	59	72	79	85	90	96	100		
Midpoint of Content Percentiles	4	18	44	66	76	83	89	94	98		
Median of Intensity Percentiles	34	39	43	44	51	64	76	83	91		

people are relatively indifferent. Very sharp V curves indicate a clear distinction between being positive and being negative.

6. THE PROBLEM OF ERROR

The technique of asking "How strongly do you feel about this?" after each question is admittedly a crude one and accounts for much of the error in the observed relationship between intensity and content. If the intrinsic intensity could be ascertained, the relationship should be perfect. How error is introduced by the technique just described can be shown in several ways. Some men would say "undecided" to a question. When asked the part (b) about strength of feeling, they say they felt "very strongly." When they were asked why they said "very

and contributed to error in the frequency distribution.

Even if all the responses were in context, there is still a contribution to error from the fact that the degrees of meaning of the words vary from sub-group to sub-group of the population. Verbal habits of people are considerably different. Some people will say "strongly agree" to almost anything when they are in favor of it, where other people would say "agree" under the same circumstances. Especially with respect to the intensity questions, there are people who say "very strongly" to every question. This tendency to use or not to use strong adjectives, we shall call "generalized verbal intensity." That it exists as a quasi-scale for the Army population has been shown in

several surveys, as will be reported in the forthcoming publication of the Research Branch. Generalized verbal intensity was measured by studying ten or so different universes of content (attitudes) in a single survey. One part (b) question on intensity

all the more that responses to any single content question must be regarded with suspicion and re-emphasizes the need for an objective technique for obtaining a zero point such as that described in this paper.

An example of how "generalized" intensity

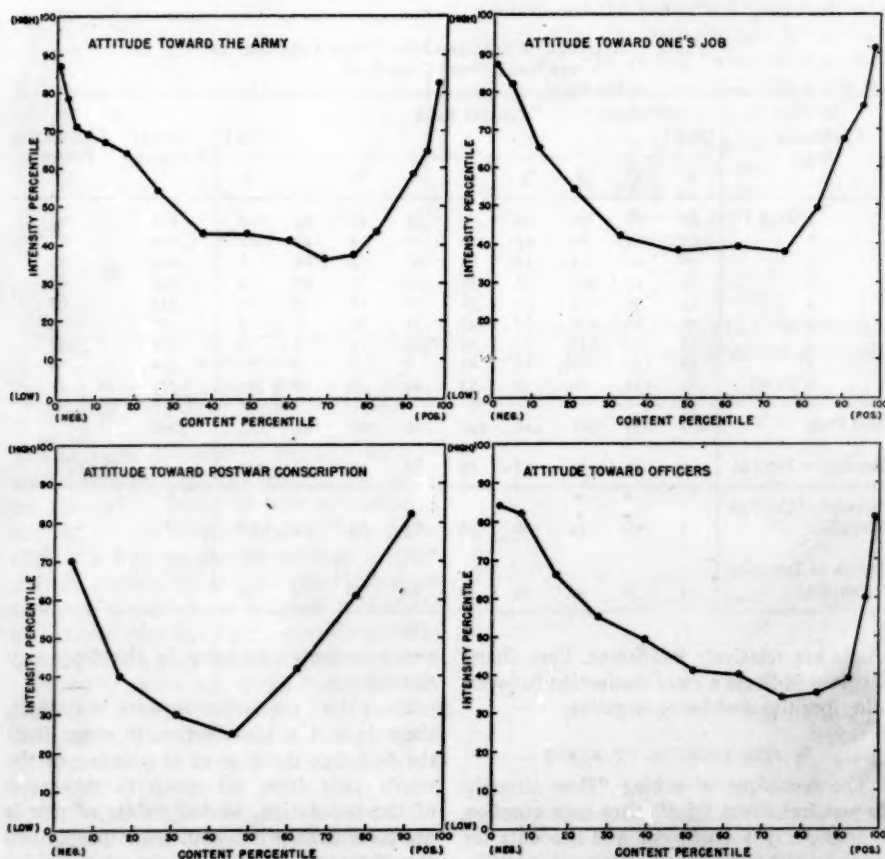


FIGURE 4. Four Examples of Approximate Intensity Curves of Attitudes of Enlisted Men.

was selected from each of the ten different areas at random, and it was found that the ten intensity questions formed a quasi-scale.

It was also found that responses to individual questions were related to this generalized intensity. This finding emphasizes

can be related to responses to a particular content question is the following. The sample of soldiers was divided into three groups according to their generalized intensity: low, medium, and high. When asked the question, "All things considered, do you think the Army is run about as

efficiently as possible, or do you think it could be run better?" the responses for the three groups turned out to be:

	General Intensity		
	Low	Medium	High
It is run as well as possible	38%	32%	28%
It could be run somewhat better	42	36	27
It could be run a lot better	20	32	45
Total Per Cent	100%	100%	100%
Total Cases	441	800	525

The categories obviously mean different things to the different kinds of people; for this reason the interpretation of any frequency distribution of responses to a single question must take into account the verbal habits of the respondents.

This "generalized" intensity, of course, is a verbal variable and reflects verbal habits of people; it is worth much further exploration. A research study was undertaken to find out if this generalized verbal variable correlated with personality areas as investigated by conventional personality inventories. Four different personality areas were studied: inferiority, hypomania, psychopathic deviate, depression. All areas correlated to some degree with generalized intensity. These personality variables were also correlated with content of specific opinion questions. This suggests a rich field of exploration of the role that personality traits play in attitudes and opinions on political, social, and economic matters, and from a methodological point of view in determining respondents' verbal habits and modes of expression.

7. SUMMARY

1. One purpose of intensity analysis is to provide an objective method of determining an invariant cutting point for an attitude or opinion scale. This cutting point will enable the research worker to divide his population into favorable and unfavorable groups, a division which will be independent

of the selection or "bias" of the specific questions asked.

2. The basis of intensity analysis is the theory of scaling which provides a test for the single meaning of a series of questions. Any single question or series of questions on an issue is considered as simply a sample of all other questions on the same issue which might have been asked instead. The problem of scaling is to test whether the particular sample of questions used can be considered as belonging to an attitude or opinion universe that contains only one dimension. Once a selected series of questions has been determined to be scalable, it is meaningful to rank the respondents from high to low on the attitude universe being studied.

3. In addition to the rank order of individuals on the content or attitude scale, it is also possible to determine the intensity with which an attitude is held. This intensity measurement is found to be a U or J shaped function of the content scale. People on both ends of the content scale feel more strongly than people towards the middle of the scale. As one moves down the content scale, intensity of feeling decreases *until a point is reached* where intensity of feeling begins to increase again. This point is invariant for any single attitude area, and regardless of the sample of attitude questions used will always divide the population into the same *proportion* with positive and negative opinions. Thus, this cutting point is both invariant and objective.

4. The method used in this paper for measuring intensity of feeling was to ask, "How strongly do you feel about this?" after each attitude question. Correlating the content scale scores with the intensity scores produced the expected U or J shaped curve. This technique of measuring intensity is far from perfect and results in a large amount of variance around the curve. Further research will undoubtedly serve to reduce much of this variance. However, crude as it is, the present technique does work and has been used successfully in many instances.

HEURISTIC AND EMPIRICAL TYPOLOGIES: A JOB FOR FACTOR ANALYSIS*

ROBERT F. WINCH
Vanderbilt University

THIS paper has two purposes: (a) to differentiate between what are called "heuristic" and "empirical" typologies, and (b) to show how the empirical typology may be derived and how it may be utilized.

It scarcely needs saying that the purpose of the typology, like that of the common noun in general, is to enable the observer to perceive order in the "infinite complexity" of the universe. For example, the abstracting of fluids from the human body as a whole, the abstracting of blood from the other bodily fluids, and the typing of blood enable both understanding and control. Typologies are created by the process of noting homogeneous attributes in heterogeneous phenomena; they are created for the purpose of discovering systems.

In terms of function and technique of derivation typologies may be classified as heuristic or empirical.¹ As contrasted with an empirical typology, a heuristic typology shows the following characteristics:

- (a) insofar as it is distinguishable from theory, it is deduced from theory;
- (b) it is constructed for the purpose of enhancing the vision of the researcher (*i.e.*, by facilitating the statement of hypotheses, the conception of testing situations, the ordering of observations);
- (c) it represents a voluntary distortion of empirical phenomena by positing extreme forms of relevant characteristics;

(d) in the logical order of things it stands between theory and the test of theory. As is implied in the foregoing, an empirical typology is derived primarily from data rather than from theory, it functions to summarize observations rather than to enhance vision or to illustrate the existence of essences, it describes modal rather than extreme characteristics, and stands logically between observation and the reformulation of theory.

Now it can be argued that the worker who uses the heuristic type is actually using data in the form of implicit, perhaps unwitting correlations, and that the user of the empirical type would be unable to formulate his typology without some sort of theory, however implicit and informal. This may be conceded, in the writer's judgment, without forfeiting the value of the distinction, as will be shown in the following remarks.

Insofar as the writers on method in our field have discussed typology they have been largely preoccupied with heuristic typology in terms of its better known name, the ideal type.² There is no intent in this paper to depreciate the ideal type. Its utility and respectability are well established. On the other hand, relatively little attention has been paid to the methodological implications of the empirical type.^{2a}

When empirical sociologists come to a situation where categorization of data is in order (*i.e.*, where the empirical typology might be used), they are disposed to use classification

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¹ Paul Wallin has classified types into "ideal" and "empirical." (See his "Supplementary Study A: The Prediction of Individual Behavior from Case Studies," pp. 181-230, esp. pp. 215-17, in Paul Horst *et al.*, *The Prediction of Personal Adjustment*, New York, Social Science Research Council, 1941.) The present writer's mild preference for "heuristic" rather than "ideal" is explained in note 2 below.

² Whether the heuristic type is identical with the ideal type or one is a sub-class of the other is a matter of definition. Since the present interest is in the more general aspects of derivation and function of the construct rather than in refinement and detail, the writer prefers to use the term "heuristic."

^{2a} A very noteworthy exception is an article by Paul F. Lazarsfeld, "Some Remarks on the Typological Procedures in Social Research," *Zeitschrift für Sozialforschung*, 6 (1937), 119-39.

rather than typology. The difference, as seen by this writer, is that classes are mutually exclusive and collectively exhaustive, and data may be distributed in the various classes. Typologies may be none of these. Observations may be "classified" in terms of a single variable, e.g., sex, marital status, or age. "Type" ordinarily connotes a plurality of correlated characteristics.^{2b} Finally, if more than one variable is used in differentiating classes, there is not assumed to be any moral burden on the researcher to show any "dynamic" relationship between them. Klüver has been particularly insistent in making the positing of an explanatory dynamic interacting system a necessary condition for a typology.³ The writer regards Klüver's conception as a universally desirable objective rather than as a necessary condition. Just where the line should be drawn between classification and typology is a definitional and hence an arbitrary matter. By following his lead we arrive at a position where a researcher's empirical categories may be called "types" or "classes" contingent upon how far and how "dynamically" the researcher proceeds in his interpretation.⁴

^{2b} Lazarsfeld uses the term "quasitype" where only one attribute is involved and reserves the term "type" for a "compound of attributes." *Op. cit.*, 120, 125. Folsom, however, observes that "type" is used to designate (a) one mode in a multimodal distribution on a single variable, or (b) "a group of correlated traits so that the possession of one or more of these traits usually implies the possession of the others." J. K. Folsom, *Social Psychology*, New York, Harper, 1931, pp. 234-35.

³ He states: "The mere description, the morphology of the type, may lead us astray in our attempt at clarifying the method. If the description refers to nothing but relations among static elements, it does not refer at all to a multiple unit, to a dynamic system, to an action-system called type. The typological method seeks to uncover certain specific modes of interaction in this system. The *conditio sine qua non* is that such modes of interacting are at least *hypothetically* assumed. Through the use of the typological method, it is true, units of some kind become visible, but these units are *dynamic* units." Heinrich Klüver, "E. Kretschmer's Study of Physique and Character," pp. 176-85, in S. A. Rice (ed.), *Methods in Social Science*, Chicago, University of Chicago Press, 1931. (Italics in original.)

⁴ Using Sorokin's categories, we might say that

A few cases of typology will be presented to illustrate the difference between the heuristic and the empirical. In the social sciences it seems clear that the most widely known heuristic type is the "economic man," a construct devised for the purpose of trying to determine what would happen "if" man sought only to maximize his profits and minimize his losses. It is apparent that no such man exists but that some individuals resemble the construct more closely than do others. Spranger⁵ uses the ideal type method in differentiating "the ideally basic types of individuality": theoretic, economic, esthetic, social, political, and religious. He states that his method

is an artifice since it allows us to study mental phenomena on a level of relatively low complexity. It must be borne in mind that the basic types which we develop are not photographs of real life but are based upon an isolating and idealising method. In this way eternal and ideal types are developed which are to be used as constructions or normal structures in connection with the phenomena of historical and social reality.⁶

A few other illustrations of the use of heuristic typologies are as follows: Weber's⁷ types of leadership, Wirth's⁸ and Handman's⁹

by virtue of their mode of derivation and function the heuristic typology is "logico-meaningful" while the empirical is "causal-functional," and that Klüver would allow us to apply the term "typology" only to the "logico-meaningful." (Cf. P. A. Sorokin, *Social and Cultural Dynamics*, Vol. I, New York, American Book Company, 1937, pp. 22-29.) The difficulty with this position is that the "meaningful" or "heuristic" typology presupposes theory and that while this arrangement is admirably suited to a research situation wherein an adequate body of theory exists, it is not very helpful in divulging relationships in the ground-breaking type of problem encountered in new problem areas.

⁵ Eduard Spranger, *Types of Men: The Psychology and Ethics of Personality*, Halle, Max Niemeyer Verlag, 1928.

⁶ *Ibid.*, p. 104.

⁷ Max Weber, *Wirtschaft und Gesellschaft*, Tübingen, J. C. B. Mohr (Paul Siebeck), 1925, pp. 122-76.

⁸ Louis Wirth, "Types of Nationalism," *American Journal of Sociology*, 41 (1936), 723-37.

⁹ Max S. Handman, "The Sentiment of Nationalism," *Political Science Quarterly*, 36 (1921), 104-21.

types of nationalism, Wirth's¹⁰ types of minority group, Benedict's¹¹ and Sorokin's¹² types of cultures, Thomas's¹³ types of personality, and Burgess's¹⁴ types of family.

One of the most celebrated empirical typologies is that of Kretschmer¹⁵ who associated affective disposition with body type. Concerning the derivation of his typology, Kretschmer states:

The types . . . are no "ideal types" which have emerged, consciously created in accordance with any given guiding principle or collection of pre-established values. They are, on the contrary, obtained from empirical sources in the following way: when a fairly large number of morphological similarities can be followed through a correspondingly large number of individuals, then we begin measuring. When we compute averages the outstanding common characteristics come out clearly, while those peculiar marks which only occur in isolated cases, disappear in the average value. In exactly the same way we treat the remainder of the characteristics which can only be described from mere optical observation. So we proceed as if we were copying at the same time the picture of one hundred individuals of a type on the same picture-surface, one on top of the other, in such a way that those characteristics which cover one another become sharply outlined, while those which do not fit over one another disappear.

¹⁰ Louis Wirth, "The Problem of Minority Groups," in Ralph Linton (ed.), *The Science of Man in the World Crisis*, New York, Columbia University Press, 1945, pp. 347-72.

¹¹ Ruth Benedict, *Patterns of Culture*, New York, Penguin Books, Inc., 1946, pp. 72 ff.

¹² P. A. Sorokin, *Social and Cultural Dynamics*, Vol. I, New York, American Book Company, 1937, pp. 72-99.

¹³ W. I. Thomas and F. Znaniecki, *The Polish Peasant in Europe and America*, New York, A. A. Knopf, 1927. This typology of personality represents a variation in the usual use of the ideal type. While the "Philistine" and the "Bohemian" are polar types, the "creative" is a second order abstraction in that it includes the socially desirable and excludes the undesirable attributes of the two polar types. Also it should be noted that Sorokin's "idealistic" culture is a mixture of the polar "ideational" and "sensitive" systems of culture.

¹⁴ E. W. Burgess and H. J. Locke, *The Family: From Institution to Companionship*, New York, American Book Company, 1945.

¹⁵ E. Kretschmer, *Physique and Character*, New York, Harcourt, Brace and Company, 1925.

Only those characteristics which become strongly marked in the average values are described as "typical." . . . Our description of types . . . refers not to the most frequent cases, but to ideal cases, to such cases as bring most clearly to view common characteristics which in the majority of instances appear only blurred, but which, all the same, can be empirically demonstrated.¹⁶

Thus it appears clear that Kretschmer's typology represents his effort to summarize his data, that it is an empirical and not a heuristic typology.

Some other examples of empirical typologies are: Angell's¹⁷ typology of the reactions of families to reduction of income, Sheldon's¹⁸ types of temperament, Ogburn's¹⁹ types of cities, and Hoxie's²⁰ types of trade unions.

There are some interesting border-line typologies. Mowrer's²¹ ecological types of family life are set forth without any suggestion as to whether they derive from theory or from data. The text makes the typology sound factual, but the suspicion persists that it is conceptual. The Jewish "social types" of Louis Wirth²² and the Negro "social types" of Samuel Strong²³ represent conceptualizations performed by the members of societies. In each study the investigator's job was to discover, describe, and

¹⁶ *Ibid.*, pp. 18-19.

¹⁷ R. C. Angell, *The Family Encounters the Depression*, New York, Scribner's, 1936, "Appendix on Method."

¹⁸ W. H. Sheldon and S. S. Stevens, *The Varieties of Temperament*, New York, Harper, 1945, esp. chapter ii.

¹⁹ W. F. Ogburn, "Social Characteristics of Cities. IX. Different Types of Cities," *Public Management*, 18 (1936), 267-73.

²⁰ R. F. Hoxie, *Trade Unionism in the United States*, New York, D. Appleton and Company, 1920, pp. 33-52.

²¹ E. R. Mowrer, *Family Disorganization*, Chicago, University of Chicago Press, 1939, pp. 110-11.

²² Louis Wirth, *The Ghetto*, Chicago, University of Chicago Press, 1928.

²³ Samuel M. Strong, *Social Types in the Negro Community in Chicago*, Chicago, University of Chicago Libraries (unpublished Ph.D. dissertation), 1940, esp. chapter xvii, and "Social Types in a Minority Group," *American Journal of Sociology*, 48 (1943), 563-73.

refine the society's conceptualizations. Kardiner's²⁴ "basic personality structures" are empirical in the sense that they are the direct result of the analysis of culture but non-empirical in the sense that they do not emerge from the study of personalities.

Despite the little amount of attention that has been devoted to empirical typologies it is the writer's judgment that they should be given a mantle of respectability because of their potential value for social research. This judgment is predicated upon the belief that empirical typologies derived by means of sufficiently powerful techniques can:

- (a) correct errors in heuristic types;
- (b) reveal types where none has been posited or suspected;
- (c) provide a basis for "integrating" various disciplines.²⁵

Of the various techniques available for the derivation of empirical typologies multiple factor analysis seems to offer the greatest promise.^{25a} This technique requires:

- (a) an aggregate of objects (persons, cultures, economies, etc.);
- (b) a set of variables;
- (c) an observation (or "test score") on each individual with respect to each variable.

The conventional mode of treatment then calls for:

- (d) determination of intercorrelations between all pairs of variables (or tests);²⁶
- (e) factoring of the resulting correlation

²⁴ Abram Kardiner, *The Individual and His Society*, New York, Columbia University Press, 1939; *The Psychological Frontiers of Society*, New York, Columbia University Press, 1945; and "The Concept of Basic Personality Structure as an Operational Tool in the Social Sciences," in Ralph Linton (ed.), *op. cit.*, pp. 107-22.

²⁵ By "integration" is meant (a) an empirical demonstration of association between data of various levels of conceptualization plus (b) a "meaningful" interpretation of this relation.

^{25a} Lazarsfeld's "reduction" of attributes to form types is a sort of informal factor analysis on dichotomized or serialized distributions. *Op. cit.*, pp. 127 ff.

²⁶ The intercorrelations are listed in a table whose columns and rows represent the tests used. This table of intercorrelations is known as the "correlation matrix."

matrix (table of intercorrelations) to determine its dimensionality;²⁷

- (f) rotation of the resulting reference vectors to such positions that they may be interpreted as "primary factors."²⁸

An example from Professor Thurstone's work²⁹ may be cited in which he uses sixty tests on 1,154 school children and isolates seven primary mental abilities: verbal comprehension, word fluency, number, space, rote memory, induction, and perception. To summarize in seven terms the substance of the performance of more than a thousand individuals on sixty tests is remarkable economy. To be sure, the act of naming by the researcher does not certify the validity of the categories. The definitions are still operational; interpretation and theorizing must remain more subjective than formal analysis. Yet the evidence is clear that "intelligence" has differentiable elements, and it is of theoretical significance that they be differentiated.

May we call these abilities "types" of cognition? Noting the definitional character of the question, Thurstone would be inclined to answer in the affirmative and to grant to anyone the right to differ on the basis of definition. As noted above, Klüver would require an integrating theory before he would permit the use of "type."

It may be held that this technique is appropriate to the investigation of intelligence but (a) that most social scientists are interested in more complicated problem areas than that of cognition, (b) that what is wanted

²⁷ I.e., to determine the number of dimensions or "factors" which will account for the common factor variance. At this juncture each of the n dimensions may be thought of as a reference vector orthogonal to all other reference vectors and each of the m intercorrelations as points in the n dimensional hyperspace. According to Thurstone the structure is not interpretable until the reference vectors are rotated to meaningful positions.

²⁸ After rotation the reference vectors are called "primary factors." At this time they may not be orthogonal. The basic work on multiple factor analysis is L. L. Thurstone, *The Vectors of Mind*, Chicago, University of Chicago Press, 1935, a new edition of which is in press.

²⁹ L. L. and T. G. Thurstone, *Factorial Studies of Intelligence*, Psychometric Monographs, No. 2, Chicago, University of Chicago Press, 1941.

is types of men or of social groups rather than types of ability, and (c) that many of the more interesting kinds of data are non-mensurable.

Related to these objections are several dilemmas which seem to be confronting social research, and particularly sociological research:

- (a) How can we effect a marriage between qualitative data, which are so difficult to analyze, and quantitative data, which are so difficult to endow with significance?
- (b) What is to be done about the pleas of certain brands of psychology and ethnology to study the "whole person" and the "whole culture"?
- (c) Is it desirable, and if desirable, is it possible to effect an integration of various disciplines in the study of a problem area (personality, labor, war, etc.)?

The old qualitative-quantitative issue cannot be settled in a few words. It should be noted, however, that people on both sides of the issue seem to be working towards each other. On the quantitative side, for example, Guttman³⁰ has been doing considerable work on the scaling problem, and Thurstone states that he and his co-workers are trying to devise a technique whereby non-mensurable data may be subjected to factor analysis. For data which are non-mensurable but are enumerable at different levels there is the analysis of variance. On the qualitative side, the thematic apperception test, one of the most promising qualitative techniques in the personality field, has been handled in a quantitative manner by Murray³¹ and Sanford.³²

Turning now to the "holistic" and "integrational" issues, the writer believes that these are different facets of the same ques-

tion. Those who wish to study a phenomenon "whole" accuse others of superficiality; those who conceptualize a problem exclusively in terms of the disciplines of their persuasion accuse the former of being hopelessly and naively "unscientific," and speak of the integration of disciplines as logically impossible. The result is mutual scoffing.

To the writer it seems clear that no object can be studied "whole." In its most complete expression the idiographic method must utilize a process of abstraction, and the term "a science of the individual" seems both paradoxical and, from the standpoint of any reasonable scientific economy, useless.

On the other hand, we are conscious of the existence of a set of problem areas, which we may regard as being in a disciplinary sense interstitial, or probably the more fruitful view is that they are transdisciplinary. Mannheim³³ has pointed out that the present disciplinary organization of the social sciences grew out of the liberalism of the late eighteenth and nineteenth centuries and was addressed to the social, political, and economic orders in terms of which life was seen at that time. It is the writer's conviction that we are becoming increasingly (though perhaps only semi-consciously) aware of the inadequacy of the nineteenth century concepts for the task of addressing twentieth century problems.³⁴ In our efforts to relate the parts and to see the problems "whole," we are recruiting teams of representatives from various disciplines. Institutes and committees of international relations, human relations, and human development, etc., are established and do research. But the integration never quite seems to come off.

³⁰ Karl Mannheim, *Man and Society in an Age of Reconstruction*, New York, Harcourt, Brace and Company, 1940, pp. 160-61.

³¹ The inadequacy of nineteenth century concepts has been noted in economics by the development of such neologisms as "duopoly," "oligopoly," and "monopsony." Cf. E. H. Chamberlin, *The Theory of Monopolistic Competition*, Cambridge, Harvard University Press, 1946, and Joan Robinson, *The Economics of Imperfect Competition*, London, Macmillan and Company, 1933.

³⁰ Louis Guttman, "A Basis for Scaling Qualitative Data," *American Sociological Review*, 9 (1944), 139-50. See also Ward H. Goodenough, "A Technique for Scale Analysis," *Educational and Psychological Measurement*, 4 (1944), 179-90.

³¹ H. A. Murray et al., *Explorations in Personality*, New York, Oxford University Press, 1938.

³² R. N. Sanford et al., *Physique, Personality and Scholarship*, Washington, Society for Research in Child Development, 1943.

What seems to be needed is new planes of conceptualization which will intersect the traditional planes. These will be the planes of the new problem areas. Factor analysis may be the technique to assist us in defining these planes. The procedure will be to begin the study of a problem area with a survey of the various conceivably relevant elements, to put these elements through a multiple factor analysis, and thereby to determine the major dimensions of the new domain. It should be noted that factor analysis does not suffer the handicap of having been trained in a single discipline, and hence it can be expected that it will show relations between data from various levels of conceptualization. It then becomes the job of the researcher to relate the factored clusters of variables into coherent theory. This is, of course, not a technical but a theoretical job; the factor analysis cannot provide the theory but it can point to the major relationships.

A typology of objects (persons, economies, cultures, etc.) is directly derivable from a variation on the usual or "direct" factor analysis technique. This variation is called "inverted" by Stephenson³⁵ and "obverse" by Thurstone. While with both techniques the original data are test scores, obverse factor analysis differs from the usual or direct technique in that correlations are run on persons rather than on tests.

To clarify this idea let us recall the appearance of a worksheet containing test data on a sample of persons. The usual arrangement is to have the test-names (e.g., digit-span and figure recognition or social status and economic status) in the column headings. In the stubs would be the names of the persons tested. In the usual procedure we should obtain our correlations by computing the sums of squares and sums of cross-products of columns. In the obverse

technique, however, we wish to correlate persons rather than tests. Hence we should compute sums of squares and of cross-products of rows rather than of columns.

Thus instead of correlating digit-span with figure-recognition or social status with economic status, one would correlate John with Ralph, Ralph with Mary, and Mary with John. Accordingly when the correlation matrix is constructed, each column and row represents a person rather than a test. The rotated reference vectors (or "primary factors") then are defined by a cluster of persons who are similar to each other and different from the others in the problem. In this way the primaries provide empirical typologies of persons. Concerning this procedure Stephenson states:

Type psychologists have grasped an important matter when they remind us that abilities and traits can be considered relative to the individual himself, and that only by so doing can the individual, so to speak, be maintained whole and intact. "The true characterological significance of the trait," says Stern, "becomes evident only when we investigate its role in the total scheme of X's character." It is precisely such an investigation that [this technique] affords.³⁶

Thus the technique gives promise of bridging the gap between the holistic and the segmental approaches.³⁷

It should perhaps be noted that while this

³⁵W. Stephenson, "The Inverted Factor Technique," *British Journal of Psychology*, 26 (1936), 351-52.

³⁶Stephenson suggests that the heterogeneity introduced by the difference in size of unit and magnitude of raw score on the different tests may be overcome by expressing each person's score on each test in terms of the sigma unit of the test. *Ibid.*, 346.

The writer is not acquainted with any literature addressed to the question of assessing the population of personal attributes from which a sample of measures is to be obtained.

Cyril Burt's view that the obverse technique divulges no more and no different information from that given by direct factor analysis seems questionable to the writer because of the fact that the examples used by him to illustrate this point are based on a very few variables. It is the writer's hypothesis that the magnitude of error in Burt's position is roughly proportional to the number of variables employed. Cf. C. Burt, "The Analysis of Temperament," *British Journal of Medical Psychology*, 17 (1938), 158-188.

³⁷W. Stephenson, "Correlating Persons instead of Tests," *Character and Personality*, 4 (1935), 17-24; "Introduction to Inverted Factor Analysis, with Some Applications to Studies in Orexia," *Journal of Educational Psychology*, 27 (1936), 353-67; "The Inverted Factor Technique," *British Journal of Psychology*, 26 (1936), 344-61.

technique would permit of using unlimited numbers of different observations, say one thousand, on each person, it would restrict the feasible size of sample of persons to from twenty to possibly fifty per problem. While this would be somewhat of a handicap and would necessitate considerable care in the selection of the sample of subjects, it would be particularly appropriate to the intensive investigation of small samples as, for example in the Murray study.

It is obvious that by means of this procedure it would be possible to study and to "type" families and voluntary associations, classes and castes, cultures and sub-cultures, economies and governments, and any other meaningful units of social research.

The question may be asked: to what kinds of sociological problems may the technique of factor analysis be applied? The first step in answering the question is to recall the two broad stages in the development of the theory of any scientific discipline: first, the stage of description, and, second, the stage of hypothesis. In other words, it is necessary to identify the principal dimensions of a problem area before it is possible to develop criteria of relevance. It is only after these criteria have been established that it becomes feasible to state a hypothesis concerning the functional relationship between *a* and *b*, other things being equal (*i.e.*, other relevant variables being held constant). Without having gone through the descriptive stage, one would be unable to state what variables should be held under control while testing a hypothesis.

Primarily, the technique of factor analysis has utility for the first, or descriptive stage. It is possible with this technique to take account of a considerable number of conceivably relevant variables (to use the "vacuum sweeper" approach), and through the factorial process to ascertain the principal dimensions of the area. Having defined the principal dimensions, the investigator is then able to state reasonable hypotheses. Factor analysis is used in the stage of hypothesis only when a typology or comparable configuration is hypothesized.

An illustration of a situation in which the factor analysis technique might have

been used is Angell's study, *The Family Encounters the Depression*.³⁸ Angell's basic technical problem was that of constructing a typology on the basis of "sympathetic insight." In "Appendix A" Angell reveals with almost unprecedented candor his mental anguish in constructing a workable typology. In his efforts to create a satisfactory typology, Angell successively employed various combinations of a considerable number of variables, such as: affection, integration of interests, symbiotic interdependence, family pride, mutual helpfulness, adaptability, change in economic functions of family members, percentage decrease in accustomed income, etc. In terms of the procedure suggested in this paper Angell might have followed one, or both, of two courses. Using direct factor analysis he might have run intercorrelations among the variables just mentioned plus any others (up to a limit of between twenty and fifty) he might have thought relevant; thereby he could have ascertained empirically the major dimensions of the problem area. Using obverse factor analysis he could have thrown into the problem all conceivably relevant variables and then he could have correlated by families instead of by variables; thereby he could have derived immediately a typology based on as complete a picture of each family as he cared to use. (It will be borne in mind that there is no limit on the practicable number of variables when the obverse procedure is used.) The obvious advantage of the latter procedure is that he would have been able to handle all variables simultaneously rather than a very small number at any one time as he actually did.

Another example of a type of problem to which factor analysis is very applicable is the analysis of the voting records of supreme court justices. The presumed objective would be to establish a typology of jurists. It is the writer's understanding that Professor Thurstone is undertaking a study of this sort.

An illustration of a situation in which factor analysis might have been used to test a hypothesized typology is presented in Shel-

³⁸ R. C. Angell, *op. cit.*

don's *The Varieties of Temperament*.³⁹ By somewhat unrigorous methods Sheldon developed three body types and three temperamental types and found a high degree of correspondence between the two typologies. It is a question of fact demonstrable by factor analysis whether or not such physical and temperamental types exist in the general population.

It is not customary to subject heuristic typologies to empirical test or even to think of doing so. That heuristic typologies might, however, be in error⁴⁰ is conceivable if we think, for example, of an ideal type of the successful small city business man. To make the error obvious let us suppose that the ideal type specifies Negroid rather than Caucasoid physical characteristics. It is clear that an empirical typology developed by factor analysis or any other procedure would correct this error. The advantage which this paper alleges for factor analysis is the ease and completeness with which the empirical typology can be constructed.

Within the writer's research experience there is an instance wherein a pair of polar ideal types of families was suggested by a set of intercorrelations. Subsequent factor analysis revealed that there were two typologies instead of one, and consequently four

types instead of two. Thereby factor analysis revealed a hitherto concealed cleavage plane.

In summary the writer would like to state his beliefs on the following points:

- (a) Empirical typologies differ from heuristic typologies in that the former emerge primarily from data and are used to summarize data. Let us try to supply them with "dynamic" systems of explanation; let us not discard them, however, if we fail in the attempt.
- (b) Empirical typologies can correct errors in heuristic typologies, can reveal types where none has been posited or suspected, and can provide a basis for "integrating" various disciplines. Empirical typologies are especially useful where the problem area is new, where the extant theory seems inadequate, and where it seems desirable to attempt a transdisciplinary approach.
- (c) By means of factor analysis it is possible to ascertain the principal dimensions of such problem areas.
- (d) It follows therefore that by means of factor analysis it is possible to develop transdisciplinary approaches to such problem areas.
- (e) By means of obverse factor analysis it is possible to develop empirical typologies of meaningful units of social research.

³⁹ W. H. Sheldon, *op. cit.*

⁴⁰ Reference is made to error in the direction of a correlation and not to the anticipated departure from reality resulting from exaggeration.

ALTERNATIVE MEANINGS OF ADJUSTMENT*

WALTER H. EATON

University of Chicago

IF ONE may judge by the frequency of its use and the central theoretical position it is often accorded, the word "adjustment" deserves to be included among the most important terms of contemporary social analysis. Certainly few terms are more often used by social scientists, or with more evident confidence in the strict empirical mean-

ing of what is being said. The purposes of the present paper are to suggest, first, that the meaning of adjustment is less clear than its common use might lead us to assume and, second, that there are alternative meanings of adjustment which, if specified and generally agreed upon, might do much to clarify both theory and research in the social sciences.

One indication of the confusion attending

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the present use of adjustment and its polar term, "maladjustment," can be found in the great number of expressions which are loosely synonymous with maladjustment. These partial synonyms include: imbalance, disorganization, dysfunction, pathology, abnormality, social problems, deviation, disequilibrium, tension, frustration, disorder, disorientation, neurosis, maladaptation, unadjustment, social disharmony, conflict, and disintegration. It may be objected that many of these terms have been introduced into social science because their innovators, dissatisfied with the previous meanings of maladjustment, have sought to free themselves from traditional obscurities by employing terms whose meanings they could themselves assign. Perhaps in the case of certain of these usages, the effort has been successful. When the investigator entertains hypotheses or encounters phenomena which the existing terms of his science are inadequate to describe, he is compelled to invent new terms.

One suspects, however, that most of these neologisms are primarily new words for old confusions. They provide their authors with distinctive titles for textbooks. They convey, in some cases, a specious sense of the greater methodological precision of the physical and biological sciences from which many were borrowed. They serve also to relieve the monotony of analyses in which crucial terms are rigorously defined and used without stylistic substitutions. What is perhaps more to the point is that this lavish use of synonyms has distracted attention from the remarkable obscurity to which the key term—adjustment—is almost invariably subject.

Kimball Young, in his *Personality and Problems of Adjustment*, may be taken as typical of this tendency. The following quotations are representative of the usages of adjustment in Young's book. "Many crucial factors determining behavior and hence adjustment lie within the individual."¹ "Many of the problems of *misunderstanding, malad-*

justment, or conflict between individuals. . . ."² "But, as we have remarked previously, *sexual adjustment* is only one phase of *successful marriage relations* and many other factors . . . affect *happiness and satisfaction* in married life. Moreover, many of these items become linked together and thus influence the *total sense of well-being and of happiness or unhappiness*."³ "We may, in fact, conceive of a scale of *adaptation or social-personal efficiency* ranging from the normal and accepted through the neurotic to the more extreme thought and behavior which we call psychotic."⁴ So far as could be discovered, Young nowhere makes explicit what he means by adjustment, even though the term is included in the title of his work.

Elliott and Merrill, in their popular textbook *Social Disorganization*, likewise attempt no systematic definition of adjustment but confine the reader's enlightenment to casual observations of which the following are typical: "When life goes along smoothly, there is obviously little difficulty in the matter of making adjustments. . . . (But) decisive events which upset the life pattern of the individual require *reorganization and readjustment*. Otherwise he becomes overwhelmed and may be acutely maladjusted."⁵ Again, referring to the onset of personal crisis, the authors observe: "In the words of Queen and Mann, this marks a period of *unadjustment*, or the first stage of *disorganization*. If the individual works out an *adequate solution*, he soon becomes adjusted and life goes on more smoothly. Occasionally, however, the multiplicity of *untoward factors* or complicating phases of the problem seems to allow no possibility for taking up the threads of the old experience and weaving a new pattern. . . . If there is no *satisfactory solution*, he enters upon a further stage of *disorganization* called *maladjustment* in which he is more

¹ *Ibid.*, p. 10.

² *Ibid.*, pp. 516, 517.

³ *Ibid.*, p. 710.

⁴ M. Elliott and F. Merrill, *Social Disorganization*, New York: Harper & Brothers, 1941, p. 77. (Italics mine.)

⁵ Kimball Young, *Personality and Problems of Adjustment*, New York: F. S. Crofts & Co., 1940, p. 6. (Italics mine.)

or less permanently *unadjusted*.⁶

It will be readily apparent that disquisitions of this sort are little more than a stringing together of folklore and tautologies. If life goes smoothly, the individual is adjusted. If not, he is usually unadjusted or even maladjusted, depending, it seems, on his ability to adjust. Fortunately, the literature of sociology contains exceptions to this semantic indifference.

In general, two broad definitions of adjustment are most frequently employed—sometimes explicitly, more often implicitly—in sociological inquiry. These definitions may be called the *attitudinal* and the *functional*.

The attitudinal definition states that the success of the individual's adjustment—and therefore the degree of his maladjustment—is determined by the extent to which he expresses himself as satisfied or dissatisfied with the manner of life he has adopted. Good adjustment, within this definition, is whatever adjustment the individual *likes*. Thus Shaffer asserts, in *The Psychology of Adjustment*, "For a person to satisfy all his motives with regard for their functioning as an interrelated system, is good adjustment."⁷ He adds that this satisfaction must be accomplished "without undue emphasis or slighting of any one motive, and . . . with consideration for the adjustments of other persons." But the source of his "individual satisfaction criterion" is clearly found in the attitudinal responses of persons whose adjustment he may be seeking to measure. In so far as we restrict ourselves to this attitudinal definition, we would ask the veteran, for example, how he likes being home and what satisfactions he has found in civilian life. We would ask the newly married person what he likes and dislikes about his marital status. We would ask the college freshman what pleasures and displeasures he has found in his new situation. In short, to meet the demands of the attitudinal criterion, we would ask the individual how he feels, and our inquiries concerning what he

does would be made in order to elicit such attitudinal responses.

The functional definition of adjustment, on the other hand, asserts that adjustment is best when the individual's behavior, apart from his attitudes toward it, most nearly conforms to the norms and expectations of the society to which he belongs. Mowrer has made this definition explicit and clear in his *Disorganization, Personal and Social*: "All personal disorganization, accordingly, represents behavior upon the part of the individual which deviates from the culturally approved norms to such an extent as to arouse social disapproval."⁸ Employing such a criterion in the study of veteran adjustment, we should first have to determine the prevailing social norms within the veteran's community. Presumably, we should go on from this to discover what the veteran's behavior actually was, and the degree of its approximation to those types of behavior which might have gained the maximum social approval. We would attempt to discover whether the veteran had entered cliques and associations of which his community approved; whether he had performed efficiently in a civilian job; whether he had avoided overt conflicts with the other members of his community.

But it will be objected, surely there is an intimate connection between behavioral norms and personal attitudes. People find pleasure and displeasure within the normative structure of social behavior, not apart from it. It is what they do, together with the social approval or disapproval occasioned by their actions, that provides the only possible source from which attitudes of pleasure and displeasure might arise. Further-

⁶E. R. Mowrer, *Disorganization, Personal and Social*, Philadelphia: J. P. Lippincott, 1942, p. 32. The difficulty of holding to a single criterion, even after it has been set forth, is illustrated by the following passage, taken from the same work: "If in the family relationship the individual's *wishes* have been so defined that they can find socially approved outlets and if the personality structure is such as to facilitate such *satisfactions*, the person achieves a high degree of social adjustment and a corresponding personal organization." *Ibid.*, p. 573. (Italics mine.)

⁷*Ibid.*, p. 78.

⁸L. F. Shaffer, *The Psychology of Adjustment*, New York: Houghton Mifflin Co., 1936, p. 138.

more, we have a reasonable conviction that the precise nature of this functioning within the social environment is responsible for whatever specific pattern of pleasures and displeasures the individual may feel.

The difficulty has been that this close relationship of attitudinal and functional adjustment is often allowed to obscure the crucial differences between them. Thus W. I. Thomas, in his book *The Unadjusted Girl*, says that "It is only as we understand behavior as a whole that we can appreciate the failure of certain (maladjusted) individuals to conform to the usual standards,"⁹ and asserts later that "We may assume also that an individual cannot be called normal [or, by implication, adjusted] in which all the four types of wishes are not satisfied in some measure and in some form."¹⁰ Many examples of this confusion might be cited. But in most cases the ambiguity involved is hidden by an absence of precise definition.

The difference between attitudinal and functional criteria of adjustment is most sharply revealed by a comparison of the research methods appropriate to data of the two kinds.

An attitudinal adjustment criterion must always refer only to the verbal reports of the person who is being studied. If a veteran, for example, tells us that he is happy, there is no higher authority to which we can resort for proof or disproof of the statement. To ask his family and friends what

they think his attitudinal adjustment may be, to observe his behavior itself, to obtain such evidence as is provided by the Rorschach or TAT,¹¹ would doubtless enable us to question the veteran more skillfully. We might turn out attention to areas of attitude which had been overlooked or only partially reported. But he himself would remain our final source of data for the attitudes of pleasure and displeasure he may have entertained.

When, in the case of functional adjustment, we are concerned not with how the veteran feels but with what he does—and in particular with the deviation of his behavior from social norms—our methods of obtaining data are essentially different. Whether we take as specific measures of functional adjustment the amount of time which the veteran spends with his family, the number of days he is idle before returning to work, or the number and kind of organizations in which he is active, we may ourselves observe the behavior to which these data refer. In practice, we are accustomed to obtain much of this information from statements made by the veteran in interviews or in responses to questionnaires; but this is a matter of research economy. In the case of attitudinal data, it is a research necessity.

If further evidence is needed to establish the difference between attitudinal and functional measures of adjustment success, it can be found in the fact that these two scores for the same person are often radically divergent.

During the writer's observation of veteran adjustment in "Midwest,"¹² many such cases were encountered. The following excerpt is from a street-corner conversation with an unmarried veteran who since his return from service has apparently been perfectly satisfied to take almost no part in the social or economic life of the community.

Interviewer: Hi, Bill, how's everything going?

¹¹ These and other affiliates of adjustment are discussed briefly below.

⁹ W. I. Thomas, *The Unadjusted Girl*, Boston: Little, Brown & Co., 1923, p. 2. (Italics mine.)

¹⁰ *Ibid.*, p. 40. This ambiguity is avoided in *The Polish Peasant* (Boston: The Gorham Press, 1920, Vol. IV, pp. 2, 3) by the distinction made between social and individual disorganization. Social disorganization is defined as "a decrease of the influence of existing social rules of behavior upon individual members of the group." Individual disorganization is defined as "a decrease in the individual's ability to organize his whole life for the efficient, progressive and continuous realization of his fundamental interests." These two types of maladjustment, Thomas there points out, have "no unequivocal connection whatever." Endless misunderstanding has arisen from the circumstance that in spite of this absence of any "unequivocal connection" the two variables are so often, in point of empirical fact, directly related.

¹² Walter H. Eaton, "Research on Veterans' Adjustment," *American Journal of Sociology*, 51: 482-487, March, 1946.

Bill: All right, all right. Yep, everything's going all right.

Interviewer: What are you planning on doing now?

Bill: Well, you know I'm working on this farm loan. (There followed an explanation of this.)

Interviewer: Have you applied for a farm loan yet?

Bill: No, I haven't. Not yet.

Interviewer: Did anything ever come of that job you were figuring you might get at the gasoline station?

Bill: Oh, no, I didn't count too heavy on that. . . .

Interviewer: Bill, how do you keep going, anyway? How do you get by?

Bill: Well, you see I get my twenty a week adjustment compensation. That lasts for fifty-two weeks, you know. Then I get my CDD pension.

Interviewer: How much is that?

Bill: Fifty-two fifty a month.

Interviewer: Well, you're all set then, aren't you?

Bill: Sure I am. I can remember when I was workin' hard and not makin' that kind of money. Hell, why work? That's the way I figure it. I know I can see my way clear till next March anyhow. Course, if I could get a good job I'd take it, but I figure I might as well look around—build myself up and take it easy. . . . Why should I take a job? I don't have many expenses—it doesn't cost me much to live, and I'm makin' more than a lot of these dumb bunnies can make workin'. Hell, I could go out and get a job any day, but I figure on waitin' till spring and seein' what kind of a deal I can get. I'm in no hurry.

What single attitudinal and functional adjustment score could be given a veteran of this type? Would it reflect his poor functional adjustment to the norms of a generally hard-working community, or would it reflect his good attitudinal adjustment—his complacent satisfaction with the socially disapproved and functionally ineffective way of life he has adopted? Again, what combined adjustment score could be given the veteran at the opposite extreme, who goes to work soon after his discharge, is respected by the community, is a considerate husband and participates actively in many organizations, and who at the same time ex-

presses dissatisfaction with much of his behavior? It seems clear that the combination of these two variables in a single, blanket adjustment score would serve only to provide us with a meaningless average, and would conceal the important problem which is actually involved. This problem is to accept the fact that attitudinal and functional adjustment may differ both in their variations and in the methods by which they are observed, and to suggest, if possible, some of their relative attributes as criteria of the individual's adjustment success.

There appear to be at least four disadvantages in using the individual's attitudinal adjustment for this purpose. First, it is impossible for us to know with certainty what the individual means by whatever symbols he uses to describe his adjustment attitudes. For example, an intensity of feeling which might be described as "very happy" by one subject might be described merely as "happy" by another. Second, we cannot be sure that the subject's report is honestly given. A recent study of veterans attending the University of Chicago revealed that those who were preparing for the ministry obtained, almost without exception, the highest attitudinal scores in the survey. It is unnecessary, of course, to question the honesty of these respondents. Yet could not a professional bias—an unwillingness to admit dissatisfaction—be responsible for the high scores obtained in this case? Third, we can never be certain that our questions have touched upon all the areas of attitude which are important to the subject. And fourth, in many of the psychoses and in certain neuroses the individual's expressions of pleasure and displeasure appear to be so dissociated or displaced that they can no longer be used in comparison with the expressions of more nearly modal persons.

Yet despite the gravity of these difficulties, some of which may be solved by the refinement of research methods,¹³ an at-

¹³ One such refinement may be found in our increasing knowledge of certain variables which, for lack of an accepted general term, can be called the *affiliates* of adjustment. Unlike the social behavior

titudinal adjustment criterion retains important advantages. Regardless of the sophistication with which some sociologists view the fact, it is the practice in everyday life—and doubtless among these same sociologists—to assume that the individual's expression of likes and dislikes is usually related to his general level of adjustment. We assess our own adjustments, and the adjustments of our acquaintances, in these terms. Organizers and administrators in every field find it useful to consult the likes and dislikes of their subordinates in assessing the well-being of the group. To suppose that adjustment cannot be measured in attitudinal terms would seem to ignore the overwhelming evidence that such estimates are not only customary but are often successfully used.

The physician who asks his patient "Where does it hurt?" or "How do you feel?" is seldom accused of relying upon "subjective" and therefore objectionable data. The physician himself would be the last to assume that expressions of satisfaction are always accompanied by the complete absence of functional pathologies. But he is also aware that attitudinal data are among the most useful available to him for determining illness and for measuring recovery. The patient who is functionally ill usually *feels* ill.

The measurement of adjustment by means of functional criteria involves both difficulties and advantages of a different order.

The difficulties seem to stem principally from the fact that social norms are far more easily described in general than in particular. In general, a social norm is a prescribed

of the individual, all of these variables can be observed in the interview or laboratory situation. Furthermore, unlike attitudinal responses, they can be measured with considerable precision. These affiliates include (a) physiological data concerning blood pressure, blood volume, muscle tremor, psychogalvanic reflexes, electroencephalograms, and rates of respiration; (b) scores on personality tests, the Rorschach test, and other projective tests; and (c) the symptomatology of neurosis. All of these affiliates, it seems, are closely related to the present attitudes and attitudinal history of the individual. As the nature of this relationship becomes clearer, the measurement of adjustment will perhaps be correspondingly simplified.

or approved way of behaving; in particular, the social norms of a society as complex and rapidly changing as our own are almost impossible to enumerate or to describe with any degree of precision. The norms of one group are seldom those of another; the norms of any group today are unlikely to be exactly the norms of the same group tomorrow. The inference is therefore admissible that in measuring adjustment by functional standards we are in constant danger of losing our criterion in the widening circles of a normative inquiry. Furthermore, by far the greatest proportion of adjustment studies are concerned with the behavior of persons in situations which are characteristically transitional. The immigrant, the veteran, the newly married couple, the parolee, the college freshman, the displaced person, the unemployed worker—all are in transit from one way of life to another. It would seem, therefore, to be a problem of utmost difficulty not only to decide which social norms are appropriate in measuring the adjustment of any given individual, but to decide, moreover, at what particular moment in his transition these measures are to be applied.

If these difficulties could be overcome, the advantages of a functional criterion are obvious. The peculiar hazards of attitudinal research might be avoided. And we might, for the first time, approach adjustment studies of more normal populations with something of the confidence of the social pathologist who can measure maladjustment in terms of crime, divorce, delinquency, and similar behavioristic phenomena.

The question suggests itself at this point—why has the distinction between attitudinal and functional adjustment been generally unrecognized? One reason may be that the theory of adjustment has often been a product of literary rather than research situations. It is a part of the "literature" in the worst sense of that term. Probably, too, it is significant that the majority of empirical adjustment studies have in fact been researches in maladjustment—criminality, delinquency, psychopathology, divorce. As we have noted, the researcher in these fields has

little difficulty in determining the meaning, for the purpose of his research, of adjustment failure. He can simply assume that divorce, psychosis, suicide, or conviction for a crime is ample evidence of maladjustment. Only in studies of more normal persons, among whom the degrees of adjustment and maladjustment are less easily detected, does the need for subtler and more rigorous criteria of adjustment success become imperative.

Yet even in empirical studies of this latter kind the crucial distinction between attitudinal and functional adjustment has been generally overlooked. In this field, the typical investigator is so involved in the analysis of interviews and questionnaire responses—and becomes so impressed in the process by the magnitude of his analytical task—that the essential difference between the two kinds of adjustment has simply escaped him. Lost in his masses of accumulated data, he is more than willing to conclude that success of adjustment has no broader meaning than a judge's rating or the score on a completed questionnaire. And having reached this conclusion, he is free to engage in a practically infinite series of statistical computations, all of which may

be entirely valid so far as the internal analysis of his data is concerned.

The inadequacy of this procedure becomes apparent when the investigator attempts to relate his study to similar studies and to more general theories of human behavior. Such relevance, it seems clear, can be established only by means of a system of general definitions which have been generally agreed upon. And it is from this system that the investigator is inevitably isolated whose concern for statistical precision exceeds his insight into the general meaning of his variables.

The present paper has sought to provide, in the distinction between attitudinal and functional adjustment, at least a basis for the system of general definitions which adjustment theory requires. It has indicated that one of the principal tasks of adjustment research is to explore the correlations of attitude and function in the adjustment process. Until these relationships have been clearly ascertained, there would seem to be little hope of answering the two grave questions: to what extent are the individuals in our society content or discontent?—and under what circumstances do these attitudes arise?

IS CONFORMITY A GENERAL OR A SPECIFIC BEHAVIOR TRAIT?*

ROBERT A. HARPER†

Ohio State University

ALTHOUGH sociologists have long concerned themselves in one way or another with conformity, neither they nor psychologists have made systematic paper and pencil studies of conforming behavior. With the exception of direct-observational studies of a very few types of situations made by a group of psychologists

under the leadership of Floyd H. Allport, our knowledge of conforming and nonconforming behavior rests largely on common-sense impressions of sociologists and social psychologists.¹

The chief weakness of the paper and pencil approach in any study of behavior is that the symbolized responses of a subject in a test situation may not truly indicate his response in a situation calling for non-sym-

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† The author wishes to express his appreciation for the helpful suggestions of Dr. Walter C. Reckless in constructing the questionnaire and conducting the investigation and for the editorial assistance of Dr. John F. Cuber in preparing the paper.

¹ For a summary of the findings of the Allport school, see Robert Chin, "An Analysis of Conformity Behavior," *Archives of Psychology*, no. 289 (1943), 46 pp.

bolized action. This weakness does not render paper and pencil studies worthless, however.

"While it is true that one of our interests in attitudes is the clue they may afford to the complete adjustment of the individual or the group, this is not their only significance. Whether a group of people actually does or does not practice permanent monogamy, the fact that it verbally declares its belief in this practice is still an aspect of its social behavior of vast significance. Its verbal expressions on the subject become perhaps the most important pressure of the social environment enforcing the monogamic sex mores in the community. The measurement of opinions, therefore, is of itself a matter of importance."²

Merton,³ Lapierre,⁴ Katz,⁵ and Newcomb⁶ have likewise contended that opinion measurement, even aside from its direct correlation with non-symbolized action, is significant. Katz's statement in regard to attitude tests would seem to be sound: "Without them (attitude tests) we are left to study the ideologies of men by intuitive methods which cannot be checked for reliability and accuracy, and we become social philosophers rather than social psychologists."⁷

In undertaking an initial questionnaire approach to the study of conformity and nonconformity, the author made an inventory of the beliefs of 504 subjects concerning twenty-five social situations. It was an attempt, in other words, to get at the *ideologies*⁸ of conformity and nonconformity in-

volved in twenty-five social situations. The investigation had three main aspects, one of which (reported upon in this paper) was an effort to shed light on the problem of whether conformity is a general behavior trait or a specific response to concrete situations. Since the whole trend of recent research in the behavioral sciences points toward the situational specificity of responses,⁹ the question of specificity seemed to be an important one to raise in reference to conformity and nonconformity.

The questionnaire is too long to include in this paper. Several examples, however, of the type of situations to which the subjects were asked to respond follow:

"It is four o'clock in the morning and you are driving through a small town on a main highway. You come to an important cross street where a traffic light has just turned red.

"a. Would you probably stop and wait for the light to turn green? ☐ yes ☐ no

"b. Would it be right or wrong for you not to stop for the red light? ☐ right ☐ wrong

"c. If you did not stop for the light, would your chances of getting caught likely be great or small? ☐ great ☐ small

"You are in school and are taking a test which you must pass in order to move into the next grade. You are 'dumb' in the subject. Just when you have about given up hope of passing, you see the answers of a good student in the next seat.

"a. Would you probably complete your test without copying from the student? ☐ yes ☐ no

"b. Would it be right or wrong for you to copy? ☐ right ☐ wrong

"c. If you did copy, would your chances of getting caught likely be great or small? ☐ great ☐ small

"You are married, and have been forced to leave your wife in order to take a job in a distant city. She will not be able to join you for about a year. You meet another woman with whom you gradually build up a close friendship. You find out that this friend would be willing to have sexual relations with you.

to political and economic systems as in the popular usage.

⁹See, e.g., Gardner Murphy, Lois B. Murphy, and Theodore M. Newcomb, *op. cit.*, p. 661-673, 864-869, 1027-1046.

²George A. Lundberg, *Social Research* (2d edition, Longmans, Green and Co., New York, 1942), p. 217.

³Robert K. Merton, "Fact and Factitiousness in Ethnic Opiniones," *American Sociological Review*, V (1940), 13-28.

⁴Richard T. Lapierre, "The Sociological Significance of Measurable Attitudes," *American Sociological Review*, III (1938), 179-181.

⁵Daniel Katz, "Attitude Measurement as a Method in Social Psychology," *Social Forces*, XV (1937), 479-482.

⁶Gardner Murphy, Lois B. Murphy, and Theodore M. Newcomb, *Experimental Social Psychology* (Harper and Brothers, New York, 1937), p. 912.

⁷Daniel Katz, *op. cit.*, p. 482.

⁸The term *ideology* is used in this paper to refer to any expressed belief concept and is not restricted

"a. Would you probably *avoid* having sexual relations with this woman? —yes —no

"b. Would it be right or wrong for you to have sexual relations with her? —right —wrong

"c. If you did have sexual relations with her, would your chances of getting caught likely be great or small? —great —small"

Since one of the groups responding to the questionnaire was composed of women, some of the situations (such as the immediately foregoing example) were modified enough to fit female social roles.

These directions preceded the presentation of the situations:

"In the following pages you will find some social situations described. After carefully reading about each situation, you are asked to answer three questions about that situation. The first (Question a.) asks you what you would probably do in that situation. The second (Question b.) asks you whether you would consider a certain kind of action in that situation right or wrong. The third (Question c.) asks you whether you think the chances of your getting caught in a certain kind of action are likely great or small. Please check (by using an X) one of the two answers for each a., b., and c. question following every situation. In case of doubt, mark with an X the answer toward which you more strongly lean. Please be honest; this is a scientific study of large groups of individuals which will in no way be used for or against any individual taking the examination."

It was impossible in the present investigation to take a systematically stratified sample of the American population. Five markedly differing groups were, however, studied: (1) male residents of a large state university dormitory, (2) a group of conscientious objectors at a Civilian Public Service Camp, (3) a group of male Catholic students of a Catholic university, (4) incoming inmates of a Federal reformatory for young men, and (5) female students of a small state university

The possible involvement of a differential race factor was avoided by using only white subjects for the test, and the sex factor was controlled by not grouping the responses of the female group with the responses of the four male groups.

Male responses were regrouped for the purpose of ascertaining whether or not age, size of community where the subjects spent most of their childhood, and the occupation of their fathers during most of their (the subjects') childhood were important. Because the total number of female respondents was only eighty, female responses were not regrouped according to these factors.

One of the situations on the inventory was repeated as a check against those who responded indiscriminately. If the responses to the two presentations were the same, the inventory was retained for further analysis. If the responses differed in any respect, the inventory was rejected.

A personal interview check was administered to 75 (or approximately 15 per cent) of the 504 subjects. A comparison of personal interview replies with inventory responses led to the conclusion that the inventory was probably about 75 per cent accurate in its indications of what the subjects taking the inventory said in the interview that they believed they would do in similar "real life" situations. Stated differently, the ideological validity of the inventory is probably approximately 75 per cent.¹⁰

In order to determine the cross-sectional consistency of individual conformity,¹¹ each subject's number of conformity answers out of the twenty-five situations was totaled. Each male individual's score was then placed in his appropriate category for age, community, and occupation, and the arithmetic mean and median computed for each grouping (see table 1).¹²

¹⁰ For a discussion of methods customarily used to determine validity and reliability of attitude scales and for an example of the application of these methods to a particular study, see Theodore M. Newcomb, *Personality and Social Change* (Dryden Press, 1943), p. 186-198.

¹¹ The term cross-sectional consistency is used to refer to the consistency with which an individual is a conformist in various situations and is to be distinguished from what may be called longitudinal consistency, or consistency of conformity to the same situation in time succession.

¹² Since the questionnaire used was only a crude beginning in the field of conformity, it was decided that there was no need to employ higher statistical techniques such as partial and multiple correlation.

It may be said of the means and medians in general that they all fall at least slightly above the half-way point of conformity and non-conformity responses in the twenty-five situations (the means range from a low of 13.3 to a high of 18.1, and the medians

treme nonconformity responses are both rare, with the latter being somewhat fewer than the former (see fig. 1).

To the question, then, whether or not conformity and nonconformity tend to be general behavior patterns, this study tentatively

TABLE I. MEANS AND MEDIANS OF CONFORMING ANSWERS BY TEST GROUPS, BY AGE, BY SIZE OF COMMUNITY WHERE SUBJECTS SPENT MOST OF CHILDHOOD, AND BY FATHER'S OCCUPATION DURING MOST OF CHILDHOOD

	Number	Mean	Median
Group I (Ohio State University male dormitory students).....	109	13.6	14.4
Group II (Merom, Indiana, Civilian Public Service Camp conscientious objectors).....	62	15.5	16.0
Group III (University of Dayton, Ohio, male Catholic students).....	87	13.9	13.9
Group IV (inmates at the Federal Reformatory, Chillicothe).....	166	13.8	15.0
Group V (women students at Kent State University, Kent, Ohio).....	80	18.1	17.4
Total.....	504		
Male Age Groupings			
19 and under.....	139	13.4	14.1
20 and 21 years.....	106	13.5	13.6
22 and 23 years.....	92	14.8	15.0
24 and over.....	76	15.0	16.9
Age not reported.....	11		
Total.....	424		
Groupings by Size of Community Where Males Spent Most of Their Childhood (First 16 Years)			
Rural farm areas and towns under 2,500 population.....	96	15.3	16.0
Towns between 2,500 and 50,000 population.....	115	14.0	15.1
Cities between 50,000 and 100,000 population.....	61	13.8	13.9
Cities of over 100,000 population.....	145	13.3	13.8
Community Size not reported.....	7		
Total.....	424		
Groupings by Occupation of Male Subjects' Fathers during Most of Subjects' Childhood			
Farmer.....	42	16.1	17.0
Unskilled worker.....	32	14.2	14.5
Skilled Worker.....	105	13.9	15.1
Clerk or salesman.....	51	14.4	13.3
Business owner.....	78	13.4	13.7
Professional activity.....	60	13.6	14.7
Father's occupation not reported.....	56		
Total.....	424		

range from a low of 13.3 to a high of 17.4). That is, the mean and median tendency in all groupings is to have more conformity responses (more than 13 out of 25 situations) than nonconformity responses.

Another factor which appears in the analysis of the groupings is that the distributions of individual scores roughly approximate a normal curve. Extreme conformity and ex-

gives a negative answer. At least from the standpoint of ideology, most of the individuals herein investigated tend to be almost as inconsistent as consistent in their responses to symbolized conformity-nonconformity situations.

While the distribution of conformity responses of all individuals tested in all the twenty-five situations covered makes it im-

possible to claim that conformity is a general trait, some groupings of the respondents indicate considerable variation in the amount of cross-sectional consistency. Such variation might be due merely to accidents of sampling, or it might be due to the selecting of particular temperaments by the various categorizations. It also seems possible that differing traditions, conduct norms, and ideologies are represented by the various groups.

be more consistently rule-obeying than men. Moreover, some of the nonconforming actions in the questionnaire (such as breaking codes in regard to drinking, spitting, etc.), while probably not highly reprehensible for the male in our society, would be judged very "unladylike."

The second highest mean and median (15.5 and 16.0) are those of the conscientious objectors (Group II). This fact is again in harmony with common observation;

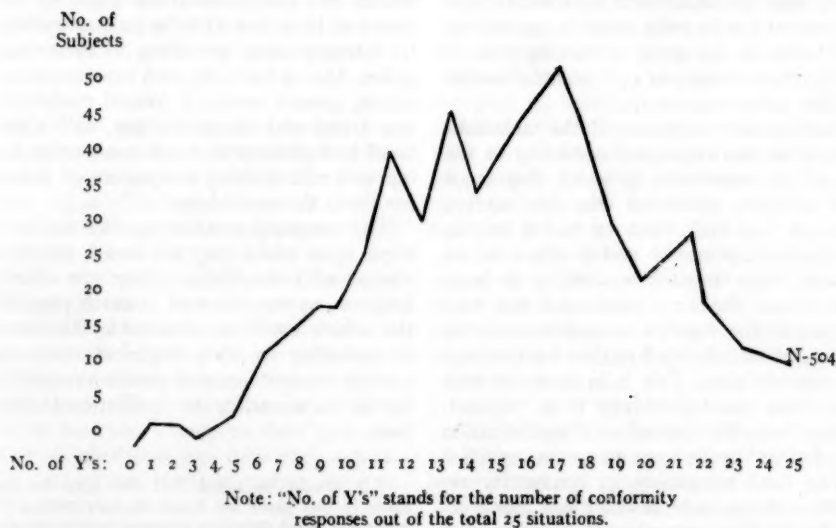


FIGURE 1. Number of subjects indicating conforming answers by number of situations (0-25)

In the absence of any technique in the present study to parcel out these possible factors, it is assumed for the present that part of the variation in the amount of cross-sectional consistency of conformity responses displayed by individuals of various groupings is due to the effect of prevailing ideologies and conduct norms on individual behavior.

Of the five test groups, the Kent State University women students (Group V in Table 1) have the highest mean and median (18.1 and 17.4, respectively), indicating the highest rate of consistency of conforming responses. This is in harmony with everyday observation that women tend in general to

namely, that *except in those situations in any way connected with military life, conscientious objectors tend to be a consistently high rule-obeying group.* This may also be taken as further support of our foregoing contention that conformity and nonconformity are specific responses to particular situations, for here we have a group labeled as extremely nonconforming appearing as such only in those situations in the questionnaire which bear upon its one particular ideological disagreement with the majority of American society.

When the groupings by age for the men are examined, a general trend of rising conformity with increasing age can be observed.

There is practically no difference between the means and medians for the group containing those 19 years of age and under (13.4 and 14.1) and for the group containing those 20 and 21 years of age (13.5 and 13.6). Although the number of cases is too small for any dogmatic assertion, it would seem possible that this is an indication that a turning point away from the "reckless" nonconformity of youth toward the more conservative conformity of maturity may lie somewhere between 21 and 22 years of age for some males in our society (the mean for the group containing men 22 and 23 years of age was 14.8, and the median for this group was 15.0).

Questionnaire responses of the male subjects were also regrouped according to the size of the community in which they spent most of their childhood (the first sixteen years of their life). Here we find a drop in the conformity means and medians as we proceed from small communities to large ones (from the 15.3 mean and the 16.0 median of the smallest communities to the 13.3 mean and the 13.8 median for the largest communities). This is in harmony with what some sociologists refer to as "emancipation from the mores" or "sophistication of behavior" in the more urban communities.

The final regrouping of conformity responses of the male subjects was made according to the occupation their fathers followed during most of their (the subjects') childhood. The hypothesis of urban emancipation or sophistication is borne out again,

for the highest mean (16.1) and the highest median (17.0) are found in the group whose fathers were farmers, and the lowest means and medians are found in the non-farming occupational groups.¹³

It may be said in summary, therefore, that conformity and nonconformity, at least at the paper and pencil level, tend to be specific responses to particular situations rather than general behavior patterns. The greater cross-sectional consistency of responses of the women and the conscientious objectors was shown to be in line with the stricter moralistic interpretations prevailing in these categories. Also in harmony with common observation, greater tendency toward conformity was found with increasing age, with childhood backgrounds of rural community living, and with farming occupations of fathers for the male respondents.

The foregoing conclusions, like the technique upon which they are based, are to be viewed with skepticism. They are offered, however, as suggestive of research possibilities which could be of considerable worth in providing us with empirical data concerning conforming and nonconforming behavior in a variety of institutional situations.

¹³It should be noted that the generalizations made in this paper are based on only twenty-five paper and pencil situations and may be thought of as applying only to those situations. It is possible, for example, that another selection of situations would fail to demonstrate a higher conformity among subjects with rural backgrounds.

THE SOCIOLOGY OF RIGHT AND WRONG*

THOMAS HANCOCK GRAFTON

Mary Baldwin College

THE SOCIOLOGY of right and wrong investigates the judgments of right and wrong which men actually make, classifying them and interpreting them in relation to the social structures and processes which condition them and into which they, in turn, enter as interactive factors.

The sociologist assumes no responsibility in regard to the ultimate validity of such judgments. He seeks to understand why men so-and-so circumstanced define particular acts as right and others as wrong, and how "right" and "wrong" affect future acts. His aim is to formulate general principles by which judgments of right and wrong may be predicted, or, if these judgments are al-

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ready known, to forecast the probable courses of action in society. He may incidentally, though not as a scientist, cherish the hope—and doubtless does—that his findings will be of value to the ethical philosopher in setting up satisfactory norms of conduct.

In the following discussion, which purports to be on the scientific level, right and wrong will be understood as designating not absolutes, or what some would say are "really so," but rather the definitions which persons in particular situations give to specified events, actual or contemplated.

I

Since right and wrong are terms in very common usage, being employed in manifold contexts by persons of every grade of education and enlightenment, there is indication of some constancy in the objects to which they refer.

Much of the research in sociology and anthropology may seem to point to the contrary. Men are not agreed as to what is right and what is wrong. Among sociologists the dictum is widely accepted that the mores can make *any* thing right while, paradoxically, it is common knowledge that even the best established practices have their critics. Right and wrong, it would appear, have no abiding city.

We propose, nevertheless, to try out the presumption that the difficulty, as in so many other instances where confusion was in the end mitigated or dispelled, lies in an unhappy choice of the variables selected as the units of analysis.

It is well to look into the meanings of right and wrong as the words occur in ordinary speech, remembering Cooley's remark that language is a psychologist that labels and treasures up the subtle aspects of the human mind.¹ Any unabridged dictionary will disclose that as an adjective right means "fit," "suitable," "correct," "convenient," "according with duty," "according with truth," "conforming to justice,"

and so on. Wrong designates the opposite of these qualities. Right also implies obligation or oughtness, while wrong—though normative too—calls to desist from, or correct, that to which it refers.

What the linguistic contexts show is that right and wrong are components and implications of interaction. They at once spring out of and define the relation which one process sustains to another process or to a whole series of processes, which may be viewed as concrete or as embodying some classifying principle. They may be styled prepositional terms inasmuch as it seems impossible to make the judgments without implying the qualifications expressed by indicators of relationship. One has only to raise such questions as, is it right to kill, to appropriate property, to disobey parents, to cut a class, to eat pork, to shave the head, or to foment a revolution, to see the necessity of relating the issues to action processes or their products. If right means suitable, then suitable for what? If it means according with duty, whose duty and to what and under what circumstances?

II

Certain crucial aspects of the sociology of right and wrong may be set forth in a series of general propositions.

Theorem 1. Right and wrong are at their source functions of acts in process. Fundamentally the business of right and wrong is to expedite action, to clear the way for *something you are doing*, to mobilize requisite resources, to summon what help is needed. Right and wrong are in their primary phase positive and negative expressions, respectively, of the need of the ongoing act to fulfil itself. They reflect the inherent need of the act for the drive to sustain itself, as well as its tractions and repulsions—those tendencies to draw certain acts into itself while turning a forbidding face toward certain other acts. An example of traction is the call of the wounded man for succor; of repulsion, the demand of the man in conference to be let alone. In both of these instances right and wrong must be communicated to other parties and enter as

¹ Charles Horton Cooley. *Human Nature and the Social Order*. New York: Charles Scribner's Sons, 1922, p. 230.

structural elements into their acts.

A corollary to this theorem, then, is that *right and wrong in their primary phase imply some sort of participation on the part of the persons who make these judgments.* They are making an event come to pass, and right and wrong define their roles in seeing it through.

When the act is ended, right and wrong in their primary phase cease to exist except as the act is rehearsed in retrospect or contemplated as a future act. But such retrospection and contemplation must be active if the peculiarly poignant right and wrong are to arise. For these do not belong to the abstractions of the ivory tower or to the vocabulary of irresponsible dalliance. They come as by prayer and fasting, where life is real and earnest. Always one is doing something—overtly, sympathetically, or imaginatively—and right and wrong with gusto are functions of that, whatever it may be.

We participate, often unconsciously, in countless acts in which our roles with their rights and wrongs are prescribed for us by social institutions and the medley of miscellaneous laws, folkways, mores, conventions, and the rest. Some people, often out of sight, are interested enough to keep these activities going, and as long as we consent to have a part in them we shall share, however grudgingly, in their requisite rights and wrongs. Where the acts seem remotely related to our interests right and wrong grow dim. And when one act in which we participate runs counter to another we experience conflict and speak of conscience. Some acts are continuous, and what we call the prescriptions of the culture have reference to those acts which go on in the collective life without a break or renew themselves without special instigation when the situations that go with them come up.

Theorem 2. Unit acts are defined in relation to other acts and organized into systems which, as controls, afford more inclusive reference-points for right and wrong. We think of the classificatory terms by which anthropologists distinguish the smaller and larger aspects of a culture. Thus Linton calls

the individual act a *trait*, conceding that it may be analyzed further into *items*, and then see traits as organized into *trait complexes* and these in turn into more inclusive *activities*.² Each category seems to add something to the sum of its parts, namely, a *Gestalt* or capacity to function as a whole. The act as a whole gives rise to characteristic rights and wrongs not to be identified with those of its parts in isolation. Indeed, the requirements of the more inclusive act may serve as criteria for judging the component acts. One frequently hears it said that such-and-such an act is wrong because unbecoming a scientist, physician, or public servant, or that such-and-such procedure is socialistic, un-Presbyterian, or un-American, as the case may be.

I should like to introduce a further classification, inspired by MacIver's distinction between self-limited and common interests.³ This will serve to distinguish the *reciprocity* system from that of the *enterprise-in-common*.

A system of reciprocities is constituted by the accommodations of acts in competition or potential conflict with one another. An act of the self-limited sort, with its tractions and repulsions, is almost certain to get into trouble with other acts. In more instances than Adam Smith would have conceded, pre-established harmony is an unwarranted expectation. Depending on their relative urgencies and strengths, acts in society win recognized "rights" and immunities, for which they pay a price in the form of duties, or concessions to other acts. The function of the reciprocity system is to maximize mutual facilitations between self-limited and group-limited acts and to minimize interference. MacIver has described the sort of thing we are talking about:

"But there are also many social relationships of a give-and-take character, in respect of which both parties are animated alike by self-limited interest. This is the level on which most trading

² Ralph Linton. *The Study of Man*. New York: D. Appleton-Century Co., 1936, p. 397-398.

³ R. M. MacIver. *Society: A Textbook of Sociology*. New York: Farrar and Rinehart, Inc., 1937, p. 27-32.

relationships take place. It is generally the level of contractual relationships, of legal relationships, of the incessant interchange of services which a highly specialized society involves. Wherever we are primarily concerned with the services rendered to us, apart from the personal values of those who render the services, we act on this level."⁴

By and large we accept the understandings which constitute the reciprocity system as right because they furnish the conditions of successful self-seeking. Cabot has written,

"Conversation, entertainment, family life, office routine, business, education, in fact almost everything that people do together, comes to have its own set of rules, which help most of us to get what we want-on-the-whole. Such rules are meant to control our rebellious tendencies to go straight for what we want when we want it, irrespective of what is ahead of us (consequences), or behind us (lessons of experience), or on either side of us (other people and their desires). To guard against such anarchy there grow up trade agreements, treaties between nations, college rules, codes of ethics, laws."⁵

It is the rules that keep us from acting irrespective of what is on either side of us in the form of other people and their desires that we have especially in mind. He that swears to his own hurt and changeth not is the hero of the reciprocity system. The unit act in this system remains *mine*, but has become re-defined in view of the fact that the situation is *ours*. And for any reciprocity system to work, the intercommunication among the participants of certain rights and wrong is required. Some of these are specific and culture-limited, some abstract and universal, belonging to the nature of the reciprocity system as such.

The system of the enterprise-in-common differs from this in that the collective act which it defines is shared in a sense transcending the give-and-take of reciprocities. To borrow MacIver's terms again,⁶ the interest is *common* rather than merely *like*: the act

itself is *ours*, not just *mine*. My unit act is to be regarded as only the medium through which a larger act belonging to all of us together finds expression. Furthermore, we must enjoy the fruits of our cooperative labors without dividing them and apportioning their parts for private consumption. The focus of this system is upon the obligation which the unit act sustains to the larger act-in-common, of which it is a phase. The unit act is a means to the act-in-common, for which it exists. The reciprocity system, on the other hand, exists, for the sake of the unit acts whose mutual accommodations compose it. In the system of the enterprise-in-common, the unit acts assimilate rights and wrongs belonging to particular concrete enterprises-in-common, plus those more abstract and universal definitions without which no enterprises-in-common could be sustained.

These two systems, of course, represent ideal types and may be embodied in varying degrees in the same concrete plurality pattern. Each type, likewise, tends to shade off into the other.

Theorem 3. Acts and action-systems are organized into hierarchies which reflect their relative strengths and define the degrees of right and wrong. The ascription of ratings⁷ to specified acts and action-complexes serves to control as well as measure the respective degrees of dominance which they exercise with reference to one another, and to prevent deadlocks between conflicting desires and obligations.

The difficulty of adjudicating the clamorous claims of conflicting acts is greatly mitigated by making the ratings functional in designated situations. You do not say that business is less important than the concert or dinner-party; what you do is to keep silent when the artist is at the piano, or to make small talk when the guests are about the table, expecting at the same time that nothing will break in on your deal in the banker's office the next morning. Many events are written into the calendar, like church on Sunday morning and the football game on Saturday afternoon, and a system

⁴ *Ibid.*, p. 28.

⁵ Richard C. Cabot. *The Meaning of Right and Wrong*. New York: The Macmillan Co., 1936, p. 57.

⁶ MacIver, *op. cit.*, p. 30.

⁷ Linton, *op. cit.*, p. 427-442.

of ratings appropriate to the occasion then goes into effect. At that the doctor may be called from his pew or the fireman from the grandstand, symbolizing the inherent urgency of some interests having no respect for time and place.

Both persons and cultures owe much of their character to the acts which are given right-of-way and lead the others. Benedict has contrasted three cultures with respect to the hierarchical positions of certain great cultural interests.⁸ Linton has a provocative discussion of variability in cultural ratings.⁹

The great theorists have divided very largely on the kinds of acts which are most fundamental for human nature and the cultural life, emphasizing in their turns sex, economics, social status, religion, war, and so on.

There is ground for arguing that some acts are in their nature more important than others, in that life could hardly go on without them. On the other hand, Linton has shown that acts which are trivial to the outside observer may assume tremendous importance as interests.¹⁰ The process which Thomas calls perseveration may lead to irrational exaggerations.¹¹ Be this as it may, the significant fact for the sociologist is that some acts, whatever the reason, are dominant over others and exert more influence over them than they receive in turn. (How long this will continue to be the case is another matter.) There is thus real value in the study of the "patterns of culture," for as descriptions of the dominant things people are doing, they afford insight into the drift of prevailing rights and wrongs.

But what we have said should not obscure the fact that any act in hand, regardless of its place in the hierarchy, may become segmentalized and for the moment isolate the mind from other interests, instigating its private judgments of right and wrong, unstable and trivial and even immoral as they may otherwise appear.

Theorem 4. The analysis of acts into their structural elements facilitates their modification or displacement by other acts, thereby instituting new rights and wrongs. Ordinarily an act unfolds without critical interest in the validity of its parts and their interconnections. Conflict between acts brings attention to the structural elements and effects the analysis. The breakdown of his act helps the actor imagine other acts incorporating one or more of the elements, and if one of these imagined acts promises a more satisfactory adjustment, he will tend to substitute it for the one contemplated or in process of execution. Likewise the recognition of questionable elements in the act tends to make the actor more susceptible to the suggestions of other persons. These may come without design in the form of acts which commend themselves just because they seem to be working. On the other hand, they may represent aggressive purposeful attempts to control the actor's behavior in the interests of other parties. We think of the salesman and the reformer analyzing our acts in the hope of discovering weaknesses to be assailed and positive aspects to be harnessed to their uses.

Talcott Parsons, who has perhaps given this matter more thought than any other sociologist, identifies end, means, conditions, and norms, in "the minimum differentiation of structural elements."¹² The attack upon the act in hand may come at any one of these points. Any one of the structural elements, too, may provide what purports to be the foundation of a substitute act. The function of the structural elements in the substitution of one act for another is manifested in the criticisms which are offered of one's behavior and the advice relative thereto. It is easy to multiply examples. Thus, assuming the end, you may criticize the means: you can hew with greater comfort and efficiency if you will use the broad-axe instead of your hatchet. Assuming the means, you may criticize the end it is made to serve: tomatoes at twenty cents a pound aren't the best buy in today's market. As-

⁸ Ruth Benedict. *Patterns of Culture*. Boston: Houghton Mifflin Co., 1934.

⁹ Linton, *op. cit.*, p. 427-442.

¹⁰ *Ibid.*

¹¹ William I. Thomas. *Primitive Behavior*. New York: McGraw-Hill Book Co., Inc., 1937, p. 8-22.

¹² Talcott Parsons. *The Structure of Social Action*. New York: McGraw-Hill Book Co., Inc., 1937, p. 732.

suming the conditions, you may disapprove of the adapted act: you should not play tennis in the rain. Or assuming the conditions once more, you may disapprove of the norm: old concepts of hospitality are out of place in modern urban life. Assuming a particular norm of promptness, you may be critical of tardiness to class.

Theorem 5. The symbolic standards by which acts are "controlled" are essentially devices of communication to secure continuity or aggrandizement to certain past acts or aspects of acts which they describe or classify or whose pre-conditions they specify.

In its origin a standard is not something apart from the act but merely a way of viewing the act itself. It is simply a version of the same right and wrong which we have seen to be functions of the act in process. The standard—verbal or other—is formulated to make sure that the act will be repeated, either by the actor or by other parties. In given instances it may symbolize, besides, the impulse inherent in the act to aggrandize itself by extending its influence to other acts and dominating them. Thus viewed, the standard is a mnemonic device enabling a person or group to hold on to and carry through acts to which there has been commitment. Like a string one ties around the finger, it serves as a reminder of unfinished tasks.

Some acts do not give rise to formal standards because they are so universal nobody has to bother about their future.

By the time somebody else's standard has come to us, the act or acts which prompted it may be no longer current. In this case it will tend to weaken but may remain alive for some while as a tenacious product of learning, reminding one, perhaps, of cut flowers which continue to emit a fragrance which belies or, alas, betokens their decay. Or, the outmoded standard may continue to function through a parasitical relation to other processes such as the legal which possess intrinsic vitality.

There may be some argument as to whether standards in general are descriptive. There is no doubt about some being so: the recipe for chocolate cake is designed to make

you go through the same motions which were observed as making Aunt Sally's so delicious. But what about those "basic principles" of conduct and procedure—so formal, abstract, and idealistic—whose births it seems sacrilegious to trace to the chambermaids of mundane process? These are descriptive too in the historical sense, but mayhap of classes of acts or the conditions of their existence. In public finance the principle of ability-to-pay is a classificatory term which categorizes certain taxes well enough established in the fiscal structure to claim rightness and possibly needing to do so in the face of die-hard opposition. The fact that there is another and quite incompatible principle, that of benefits-received, functioning alongside of it, "governing" the imposition of other taxes, would seem to dispose of any transcendental logic out of which these formulas sprang ready-made and self-authenticating. Is it right for physicians to do charity work for the indigent while soaking the wealthy patients? No doubt it is, and no doubt there can be produced some majestic principle which controls the practice. But very few of the middle-class persons of my acquaintance. I suspect, would consent to the grocer invoking the principle in charging them for eggs.

Even the Golden Rule, when translated into concrete specifications, serves only to dramatize the less readily remembered side of the reciprocity system or, if it goes beyond this, to concede to others treatment of the same description one finds it right—considering what he doing—to demand for himself.

Some standards, like democracy, liberty, equality, love, and the general welfare, seem to imply too much idealization, too much orientation toward the distant future, to be interpreted as functions of past acts. It is to be remembered, however, that certain acts engender an enthusiasm causing them for the time to demand the green light in all directions. One may derive such satisfaction from love in the intimate circle that he thinks to make it universal. Sometimes, too, in defending a controvertible position, one finds it useful to affirm that the universe is of a piece with his interests. But these principles, in sober afterthought, are not intended to hold

the field. Those who declared our independence of England found it self-evident that all men were created free and equal, but they did not think that women and slaves qualified. When their fervor cooled off, they saw the true light. I am reminded of one of my former students who decried all social class distinctions and contended that there was perfect equality in her town of fifteen thousand Pennsylvanians, only to complain in the same class session that the servants in her community wanted to eat at the table with their mistresses. Our more lively acts do have a way of encroaching upon other acts, and the loftier principles memorialize their fervor. Lest all this seem cynical, it is worth remarking that Commodore Vanderbilt could not have intended universality for the standard which arose out of the exigencies of business competition: *The public be damned.*

Theorem 6. Re-definition of right and wrong is a notable factor in personal and social change. Right and wrong direct the act, providing as it were its specifications, leading it and controlling it. But more than this, *they are symptoms of an act or acts now or some-time in process.* If you hear me speak of new rights and wrongs, it is possibly not yet my act which they reflect, but in this case they are to be regarded as evidences of the impetus other acts are communicating to mine. And they will soon assimilate and control my act unless other influences interfere or my act springs to counter-attack and affirms its own reading of right and wrong. On the other hand, if I indicate that I have *changed my mind* in regard to right and wrong, it is the sign of a new act already in my repertory or in process of emergence, and displacing or modifying one or more older acts. Thus to say that the re-definition of right and wrong is a factor in personal and social change is not the same as to say that a disembodied ideal something is disarranging the world of hard facts, but means instead that one act is working on another act, or that there is interaction between a number of acts looking toward a new structural equilibrium.

There is a sort of shift in right and wrong which signifies no real structural change. The acts of a person or culture have achieved only

a relative logical integration, and there is something of the Dr. Jekyll and Mr. Hyde in all of us. We may swing to what would seem to be incompatible acts and stand for incompatible rights and wrongs. But this would only mean, as we say in a more moralistic vein, that we are disorganized, or that we have failed to meet the requirements of some observer's logic.

III

There remain a few considerations which have been by-passed in the interest of keeping the exposition in balance. And there are, in addition, the usual questions which obtrude upon any general discussion of ethical matters and which may disclose more economically than further elaboration of the author's argument the nature and tendency of his point of view. We propose to gather up what occur to us to be the most fundamental of these in a catechism of right and wrong. This will serve to clarify our theory and dramatize its implications. The answers will be given as concisely as possible and presuppose acquaintance with the foregoing presentation.

1. Is this theory general or sociological?

The sociological orientation is definitely implied in the choice of *the act* as the basic concept. In the broad sense, all acts occur within society and bear more or less obviously the social-cultural impress. The pattern of the act, the valuations put upon it, and the instigations and rationalizations that go with it are with few exceptions functions of the life of inter-communication. Right and wrong are largely significant, too, in abetting the act in hand as it encroaches upon, or defends itself against, the acts of others. Many acts are collective in that they are constituted in cooperation. But the theory is general in purporting to explain as well right and wrong in situations which may be argued as being non-social, and Robinson Crusoe would have found use for these definitions in his life in isolation. The theory seems applicable, too, in respect to the private definitions, however derived, of the mentally-dis-eased.

2. Is right limited to what are usually considered moral matters, or is it a function

of all activity? The latter is the case. Men show no hesitation in applying right and wrong to ways of holding a fork, kissing a girl, picking a pocket, making a speech, performing the duties of public office, worshipping God. Of course they do not consciously define every act as right or wrong in the process of its execution, but may do so when it is challenged or runs into some obstacle.

3. *Do right and wrong apply to intention and reverie as apart from overt acts?* There is a strong tendency for these judgments to extend to what is inward and implicit. For (a) they may be aspects of acts with an overt phase, and as such are right or wrong with the overt behavior to which they belong; or (b) they are acts in themselves though covert, and when sympathetically perceived are defined like other acts; or (c) they are recognized as leading to overt acts which are right or wrong.

4. *Why are there so many versions of the right?* Because men do so many various things, and these are either incompatible with each other or are apparently so or just different. Lincoln Steffens observed, "No general ethical principle known to me held in practice; or could hold. Only special, professional ethics limited the conduct of men, and these differed so fundamentally that a 'good merchant,' like Mayor Strong of New York, might be a 'bad politician.'"¹³ Besides the more active definitions of right there are usually others on the verbal level, the echoes in tradition of acts belonging to other days, dimming out but not yet inaudible.

5. *What is the relation of right and wrong to social structure?* Social structure is well defined as "a system of co-ordinated activities fitted to the conditions."¹⁴ It is simply a collective act, or a collective view of interaction, hard, doubtless, to perceive because it is never visible in its entirety at any one time, parts of it being latent or distant in space. Much of social structure is out of sight, too, because it consists of subjective,

implicit acts which can only be apprehended sympathetically and through "understanding." But right and wrong are functions of social structure in precisely the same way as of any other act.

6. *Are events and things which cannot be helped subject to judgments of right and wrong?* Yes, the weather may be just right for hunting; the paretic may have wrong ideas about the date. Things and events, like responsible acts, are right or wrong from the point of view of the act in hand which is affected by them. It is in point, however, to observe that these judgments do not necessarily imply reward and punishment. These are often quite ineffective as controls. It is irrational to censure what absolutely lies beyond our control: we do not scold or threaten the tides. What happens, usually, is that praise and reward, or censure and punishment, are shifted to acts which are capable of response. We do not punish a dangerous animal, but we may do something to the careless owner. We do not, if we are rational, berate the untoward weather, but may fire the meteorologist for bungling his calculations.

7. *Is there any such thing as right in general?* What is commonly referred to as "the right" is the body of established and currently accepted prescriptions in law, mores, manners, and so forth. This will vary with the culture. The actual, more specific definitions, it may be added, will vary also with one's position in the social structure and with what he is currently doing. We have heard the Solid Citizen one moment denouncing the government for interference and the next demanding what the government is going to do about high rentals and shortages in shirts.

Sometimes the moralists speak confidently of the right and wrong that are rooted in the nature of things. Apparently what they have in mind is some very abstract view of the requirements of the larger action-systems mentioned in our discussion of Theorem 2. There are certain virtues which belong to any act just because it is an act. We may call these general virtues, and think of initiative, drive, carefulness, prudence, patience, intelligence, and hope, to mention a few. Then there are

¹³ Lincoln Steffens. *The Autobiography of Lincoln Steffens*. New York: Harcourt, Brace and Co., 1931, p. 408.

¹⁴ Charles Horton Cooley. *Social Process*. New York: Charles Scribner's Sons, 1918, p. 19.

virtues which make the reciprocity system work, like honesty, truthfulness, fair play and justice, and lawfulness. Finally, the successful prosecution of the enterprise-in-common presupposes a few distinct requirements of its own such, perhaps, as loyalty and service and self-sacrifice. All this provides an intriguing vista of a Promised Land which sociological analysis will lead the way in occupying.

8. *How do unpopular and irrational rules arise?* At the time of their formulation they were neither unpopular nor irrational to the parties responsible for them, or not enough so to keep them from being made and used. If now obnoxious, it may be because they stood for differential, private interests instead of the general welfare. The rules, on the other hand, may have been correlative with some larger view of the needs of the collectivity—a view which was not generally communicated to the constituency. There is also the more obvious possibility that social change has made the old rule irrational and obsolete. Finally, dissatisfaction with the rules may be due to some segmental interest which has gotten out of hand and become intolerant of restrictions safeguarding other functions.

9. *Why are social problems so hard to solve?* Not all of them are. Where technical facilities are available and there is a basic consensus on the undesirability of present conditions plus agreement as to what is needed, solutions are pretty certain to get into motion.¹⁵ The more stubborn social problems are constituted through the conflict of competing action patterns, each of which is too well established in the general life to yield to its challengers, with no acceptable pattern in sight which is capable of transcending and reconciling the differences. Davis has shown, in his convincing study of the forms of illegitimacy, how, for example, recognition of a child born out of father-daughter incest would introduce incompatible statuses into the structure of the family.¹⁶ "Mass attitudes toward illegitimacy," he writes, "are not

accidents, but part of a functioning structure. If they condemn illegitimacy it is because the latter somehow runs contrary to the approved institutional machinery."¹⁷ The conclusion is that each type, as well as illegitimacy in general, seems susceptible to a structural-functional interpretation. Society is unwilling to give up its cherished norms, which are too tenacious to be greatly modified, but not effective enough to bring everybody into line. So, all their days, there is war between the official norms and the recalcitrant action patterns, concern over which is what we call our social problems.

10. *Does might make right?* Right and wrong are expressions of the urgency of acts. Might refers to an act of such dominance as to force adaptation if not consent upon other acts. From such might in action, there emanate definitions of right and wrong designed to win consent from acts which bar its way. If these on their part are well organized and supported by a system of inter-locking rights and wrongs, they may offer a formidable opposition in the mass, having all the power of custom and sentiment behind them. Whether at any moment might has made right depends on whether you have your eye on the balance of power in the field of overt action and formal adaptations or upon the more informal, under-cover, and covert types of response. If the latter are active and defiant, they will keep current the definition that the might which bears upon them is wrong, and when conditions change they will translate their covert rights into evident forms of might. To the extent, of course, that the claims of might are accepted and acted upon, might has made right.

Might never gets so strong that it can afford completely to be without friends. Might likes to make itself appear legal, above-board, and just; the sheep's clothing which it dons is a tribute to the power right is capable of mobilizing. Might never entirely escapes the spectre of internal dissent and revolution: the act which it supports feels the pressure stemming from alternative acts that are inherently more adaptable, that is, more

¹⁵ Richard C. Fuller and Richard R. Myers. *The Natural History of a Social Problem. American Sociological Review*, 6, p. 320-329.

¹⁶ Kingsley Davis. *The Forms of Illegitimacy. Social Forces*, 18, p. 84-85.

¹⁷ *Ibid.*, p. 77.

in line with environing acts and their definitions of right. Might that is freely transferable, like that of money and military force, is therefore less stable in its action pattern than the less obvious forms which are more integrally rooted in their patterns—habit and sentiment and the great collective interests.

The ultimate relation between might and right is thus a question of tenses and of scope of view. For the moment, if your energies are absorbed by your mighty act and you have no conflicts, then for you might is right and you can drive ahead, heedless of the rights of others. As long as others oppose, even covertly, there exists a reference-point making your might wrong. And your might may be wrong if you look to the future where, under changed conditions within and without, it must bend before other rightful acts.

11. *How do right and wrong change?* Making some allowance for the persistence of right and wrong as verbal tradition, right and wrong change, basically, as acts change. The task of explaining changes in action patterns is the responsibility of all the sciences that deal with individual and collective behavior.

12. *Will right triumph at last?* This is a matter for faith rather than science, but we may re-state the question in terms of our theory and hope for some clarification of the processes involved. There are really three questions, any one of which may be implied: (1) Will the concrete action patterns in which I now participate or with which I sympathize

survive in the struggle for existence and secure general participation? (2) Will the acts of men become so generally harmonized in the course of time that what they then call right will be universally accepted as such? (3) Will the growing appreciation of the necessities of action and especially of collective action win universal recognition for what we have called general virtues, virtues of reciprocity, and virtues of the enterprise-in-common?

13. *What are the implications of this analysis for ethical theory and reform?* The primacy of act and inter-action and the functional nature of morals; the necessity of starting from current acts and action-systems already satisfactory to the reformers; analysis of these and of their implication for other acts-to-be-controlled, and the formulation of these implications as rights and wrongs; education of persons now participating in the approved acts to their implications and consequent rights and wrongs; identification and definition of other concrete acts and institutional structures inter-related with those in hand as right or wrong; the aggressive diffusion of these definitions with accompanying demands for change; the mobilization of power to support these definitions and demands; respect the while for the abstract virtues of reciprocity and common enterprise which will be needed to sustain the rising right, even though now largely serving the vested wrong.

WHO IS THE CRIMINAL?*

PAUL W. TAPPAN

New York University

WHAT IS CRIME? As a lawyer-sociologist, the writer finds perturbing the current confusion on this important issue. Important because it delimits the subject matter of criminological investigation. A criminologist who strives to aid in formulating the beginnings of a science finds himself in an increasingly equivocal position. He studies the criminals convicted by the courts and is then confounded by the growing clamor that he is not studying the real criminal at all, but an insignificant proportion of non-representative and stupid unfortunates who happened to have become enmeshed in technical legal difficulties. It has become a fashion to maintain that the convicted population is no proper category for the empirical research of the criminologist. Ergo, the many studies of convicts which have been conducted by the orthodox, now presumably outmoded criminologists, have no real meaning for either descriptive or scientific purposes. Off with the old criminologies, on with the new orientations, the new horizons!

This position reflects in part at least the familiar suspicion and misunderstanding held by the layman sociologist toward the law. To a large extent it reveals the feeling among social scientists that not all anti-social conduct is proscribed by law (which is probably true), that not all conduct violative of the criminal code is truly anti-social, or is not so to any significant extent (which is also undoubtedly true). Among some students the opposition to the traditional definition of crime as law violation arises from their desire to discover and study wrongs which are absolute and eternal rather than mere violations of a statutory and case law system which vary in time and place; this is essentially the old metaphysical search for the law of nature. They consider the dynamic and relativistic nature of law to be a barrier to

the growth of a scientific system of hypotheses possessing universal validity.¹

Recent protestants against the orthodox conceptions of crime and criminal are diverse in their views: they unite only in their denial of the allegedly legalistic and arbitrary doctrine that those convicted under the criminal law are the criminals of our society and in promoting the confusion as to the proper province of criminology. It is enough here to examine briefly a few of the current schisms with a view to the difficulties at which they arrive.

I

A number of criminologists today maintain that mere violation of the criminal law is an artificial criterion of criminality, that categories set up by the law do not meet the

¹ The manner in which the legal definition of the criminal is avoided by prominent sociological scholars through amazingly loose, circumlocutory description may be instanced by this sort of definition: "Because a collective system has social validity in the eyes of each and all of those who share in it, because it is endowed with a special dignity which merely individual systems lack altogether, individual behavior which endangers a collective system and threatens to harm any of its elements appears quite different from an aggression against an individual (unless, of course, such an aggression hurts collective values as well as individual values). It is not only a harmful act, but an objectively evil act [sic!], a violation of social validity, an offense against the superior dignity of this collective system. . . . The best term to express the specific significance of such behavior is crime. We are aware that in using the word in this sense, we are giving it a much wider significance than it has in criminology. But we believe that it is desirable for criminology to put its investigations on a broader basis; for strictly speaking, it still lacks a proper theoretic basis. . . . Legal qualifications are not founded on the results of previous research and not made for the purpose of future research; therefore they have no claim to be valid as scientific generalizations—nor even as heuristic hypotheses." Florian Znaniecki, "Social Research in Criminology," *12 Sociology and Social Research* 207, (1928).

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demands of scientists because they are of a "fortuitous nature" and do not "arise intrinsically from the nature of the subject matter."² The validity of this contention must depend, of course, upon what the nature of the subject matter is. These scholars suggest that, as a part of the general study of human behavior, criminology should concern itself broadly with all anti-social conduct, behavior injurious to society. We take it that anti-social conduct is essentially any sort of behavior which violates some social interest. What are these social interests? Which are weighty enough to merit the concern of the sociologist, to bear the odium of crime? What shall constitute a violation of them?—particularly where, as is so commonly true in our complicated and unintegrated society, these interests are themselves in conflict? Roscoe Pound's suggestive classification of the social interests served by law is valuable in a juristic framework, but it solves no problems for the sociologist who seeks to depart from legal standards in search of all manner of anti-social behavior.

However desirable may be the concept of socially injurious conduct for purposes of general normation or abstract description, it does not define what is injurious. It sets no standard. It does not discriminate cases, but merely invites the subjective value-judgments of the investigator. Until it is structurally embodied with distinct criteria or norms—as is now the case in the legal system—the notion of anti-social conduct is useless for purposes of research, even for the rawest empiricism. The emancipated criminologist reasons himself into a cul de sac: having decided that it is footless to study convicted offenders on the ground that this is an artificial category—though its membership is quite precisely ascertainable, he must now conclude that, in his lack of standards to determine anti-sociality, though this may be what he considers a real scientific category, its membership and its characteristics are unascertainable. Failing to define anti-social behavior in any fashion suit-

able to research, the criminologist may be deluded further into assuming that there is an absoluteness and permanence in this undefined category, lacking in the law. It is unwise for the social scientist ever to forget that all standards of social normation are relative, impermanent, variable. And that they do not, certainly the law does not, arise out of mere fortuity or artifice.³

II

In a differing approach certain other criminologists suggest that "conduct norms" rather than either crime or anti-social conduct should be studied.⁴ There is an unquestionable need to pursue the investigation of general conduct norms and their violation. It is desirable to segregate the various classes of such norms, to determine relationships between them, to understand similarities and differences between them as to the norms themselves, their sources, methods of imposition of control, and their consequences. The subject matter of this field of social control is in a regrettably primitive state. It will be important to discover the individuals who belong within the several categories of norm violators established and to determine then what motivations operate to promote conformity or breach. So far as it may be determinable, we shall wish to know in what way these motivations may serve to insure conformity to different sets of conduct norms, how they may overlap and reinforce the norms or conflict and weaken the effectiveness of the norms.

We concur in the importance of the study of conduct norms and their violation and,

² An instance of this broadening of the concept of the criminal is the penchant among certain anthropologists to equate crime with taboo. See, especially, Bronislaw Malinowski, *Crime and Custom in Savage Society*, (1936), and "A New Instrument for the Study of Law—Especially Primitive," 51 *Yale L. J.* 1237, (1944). Compare William Seagle, "Primitive Law and Professor Malinowski," 39 *American Anthropologist* 275, (1937), and *The Quest for Law*, (1941). Karl Llewellyn and E. Adamson Hoebel, *The Cheyenne Way*, (1941) and E. Adamson Hoebel, "Law and Anthropology," 32 *Virginia L. R.* 835, (1946).

³ Sellin, *op. cit.*, pp. 25 ff.

⁴ See, for example, Thorsten Sellin, *Culture Conflict and Crime*, pp. 20-21, (1938).

more particularly, if we are to develop a science of human behavior, in the need for careful researches to determine the psychological and environmental variables which are associated etiologically with non-conformity to these norms. However, the importance of the more general subject matter of social control or "ethology" does not mean that the more specific study of the law-violator is non-significant. Indeed, the direction of progress in the field of social control seems to lie largely in the observation and analysis of more specific types of non-conformity to particular, specialized standards. We shall learn more by attempting to determine why some individuals take human life deliberately and with premeditation, why some take property by force and others by trick, than we shall in seeking at the start a universal formula to account for any and all behavior in breach of social interests. This broader knowledge of conduct norms may conceivably develop through induction, in its inevitably very generic terms, from the empirical data derived in the study of particular sorts of violations. Too, our more specific information about the factors which lie behind violations of precisely defined norms will be more useful in the technology of social control. Where legal standards require change to keep step with the changing requirements of a dynamic society, the sociologist may advocate—even as the legal profession does—the necessary statutory modifications, rather than assume that for sociological purposes the conduct he disapproves is already criminal, without legislative, political, or judicial intervention.

III

Another increasingly widespread and seductive movement to revolutionize the concepts of crime and criminal has developed around the currently fashionable dogma of "white collar crime." This is actually a particular school among those who contend that the criminologist should study anti-social behavior rather than law violation. The dominant contention of the group appears to be that the convict classes are merely our "petty" criminals, the few whose

depredations against society have been on a small scale, who have blundered into difficulties with the police and courts through their ignorance and stupidity. The important criminals, those who do irreparable damage with impunity, deftly evade the machinery of justice, either by remaining "technically" within the law or by exercising their intelligence, financial prowess, or political connections in its violation. We seek a definition of the white collar criminal and find an amazing diversity, even among those flowing from the same pen, and observe that characteristically they are loose, doctrinaire, and invective. When Professor Sutherland launched the term, it was applied to those individuals of upper socioeconomic class who violate the criminal law, usually by breach of trust, in the ordinary course of their business activities.⁵ This original usage accords with legal ideas of crime and points moreover to the significant and difficult problems of enforcement in the areas of business crimes, particularly where those violations are made criminal by recent statutory enactment. From this fruitful beginning the term has spread into vacuity, wide and handsome. We learn that the white collar criminal, may be the suave and deceptive merchant prince or "robber baron," that the existence of such crime may be determined readily "in casual conversation with a representative of an occupation by asking him, 'What crooked practices are found in your occupation?'"⁶

Confusion grows as we learn from another proponent of this concept that, "There are various phases of white-collar criminality that touch the lives of the common man almost daily. The large majority of them are operating within the letter and spirit of the law. . . ." and that "In short, greed, not need, lies at the basis of white-collar crime."⁷ Apparently the criminal may be

⁵ E. H. Sutherland, "Crime and Business," 217 *The Annals of the American Academy of Political and Social Science* 112, (1941).

⁶ Sutherland, "White-Collar Criminality," 5 *American Sociological Review* 1, (1940).

⁷ Harry Elmer Barnes and Negley K. Teeters, *New Horizons in Criminology*, pp. 42-43, (1943).

law obedient but greedy; the specific quality of his crimes is far from clear.

Another avenue is taken in Professor Sutherland's more recent definition of crime as a "legal description of an act as socially injurious and legal provision of penalty for the act."⁸ Here he has deemed the connotation of his term too narrow if confined to violations of the criminal code; he includes by a slight modification conduct violative of any law, civil or criminal, when it is "socially injurious."

In light of these definitions, the normative issue is pointed. Who should be considered the white collar criminal? Is it the merchant who, out of greed, business acumen, or competitive motivations, breaches a trust with his consumer by "puffing his wares" beyond their merits, by pricing them beyond their value, or by ordinary advertising? Is it he who breaks trust with his employees in order to keep wages down, refusing to permit labor organization or to bargain collectively, and who is found guilty by a labor relations board of an unfair labor practice? May it be the white collar worker who breaches trust with his employers by inefficient performance at work, by sympathetic strike or secondary boycott? Or is it the merchandiser who violates ethics by under-cutting the prices of his fellow merchants? In general these acts do not violate the criminal law. All in some manner breach a trust for motives which a criminologist may (or may not) disapprove for one reason or another. All are within the framework of the norms of ordinary business practice. One seeks in vain for criteria to determine this white collar criminality. It is the conduct of one who wears a white collar and who indulges in occupational behavior to which some particular criminologist takes exception. It may easily be a term of propaganda. For purposes of empirical research or objective description, what is it?

Whether criminology aspires one day to become a science or a repository of reasonably accurate descriptive information, it can-

not tolerate a nomenclature of such loose and variable usage. A special hazard exists in the employment of the term, "white collar criminal," in that it invites individual systems of private values to run riot in an area (economic ethics) where gross variation exists among criminologists as well as others. The rebel may enjoy a veritable orgy of delight in damning as criminal most anyone he pleases; one imagines that some experts would thus consign to the criminal classes any successful capitalistic business man; the reactionary or conservative, complacently viewing the occupational practices of the business world might find all in perfect order in this best of all possible worlds. The result may be fine indoctrination or catharsis achieved through blustering broadsides against the "existing system." It is not criminology. It is not social science. The terms "unfair," "infringement," "discrimination," "injury to society," and so on, employed by the white collar criminologists cannot, taken alone, differentiate criminal and non-criminal. Until refined to mean certain specific actions, they are merely epithets.

Vague, omnibus concepts defining crime are a blight upon either a legal system or a system of sociology that strives to be objective. They allow judge, administrator, or—conceivably—sociologist, in an undirected, freely operating discretion, to attribute the status "criminal" to any individual or class which he conceives nefarious. This can accomplish no desirable objective, either politically or sociologically.⁹

⁹ In the province of juvenile delinquency we may observe already the evil that flows from this sort of loose definition in applied sociology. In many jurisdictions, under broad statutory definition of delinquency, it has become common practice to adjudicate as delinquent any child deemed to be anti-social or a behavior problem. Instead of requiring sound systematic proof of specific reprehensible conduct, the courts can attach to children the odious label of delinquent through the evaluations and recommendations of over-worked, under-trained case investigators who convey to the judge their hearsay testimony of neighborhood gossip and personal predilection. Thus these vaunted "socialized tribunals" sometimes become themselves a source of delinquent and criminal careers as they adjudge individuals who are innocent of proven wrong to

⁸ Sutherland, "Is 'White-Collar Crime' Crime?" in *American Sociological Review* 132, (1945).

Worse than futile, it is courting disaster, political, economic, and social, to promulgate a system of justice in which the individual may be held criminal without having committed a crime, defined with some precision by statute and case law. To describe crime the sociologist, like the lawyer-legislator, must do more than condemn conduct deviation in the abstract. He must avoid definitions predicated simply upon state of mind or social injury and determine what particular types of deviation, in what directions, and to what degree, shall be considered criminal. This is exactly what the criminal code today attempts to do, though imperfectly of course. More slowly and conservatively than many of us would wish: that is in the nature of legal institutions, as it is in other social institutions as well. But law has defined with greater clarity and precision the conduct which is criminal than our anti-legalistic criminologists promise to do; it has moreover promoted a stability, a security and dependability of justice through its exactness, its so-called technicalities, and its moderation in inspecting proposals for change.

IV

Having considered the conceptions of an innovating sociology in ascribing the terms "crime" and "criminal," let us state here the juristic view: Only those are criminals who have been adjudicated as such by the courts. Crime is an intentional act in violation of the criminal law (statutory and case law), committed without defense or excuse, and penalized by the state as a felony or misdemeanor. In studying the offender there can be no presumption that arrested, arraigned, indicted, or prosecuted persons are criminals unless they also be held guilty beyond a reasonable doubt of a particular offense.¹⁰

a depraved offender's status through an administrative determination of something they know vaguely as anti-social conduct. See Introduction by Roscoe Pound of Pauline V. Young, *Social Treatment in Probation and Delinquency*, (1937). See also Paul W. Tappan, *Delinquent Girls in Court*, (1947) and "Treatment Without Trial," 24 *Social Forces*, 306, (1946).

¹⁰ The unconvicted suspect cannot be known as

Even less than the unconvicted suspect can those individuals be considered criminal who have violated no law. Only those are criminals who have been selected by a clear substantive and a careful adjective law, such as obtains in our courts. The unconvicted offenders of whom the criminologist may wish to take cognizance are an important but unselected group; it has no specific membership presently ascertainable. Sociologists may strive, as does the legal profession, to perfect measures for more complete and accurate ascertainment of offenders, but it is futile simply to rail against a machinery of justice which is, and to a large extent must inevitably remain, something less than entirely accurate or efficient.

Criminal behavior as here defined fits very nicely into the sociologists' formulations of social control. Here we find *norms* of conduct, comparable to the mores, but considerably more distinct, precise, and detailed, as they are fashioned through statutory and case law. The *agencies* of this control, like the norms themselves, are more formal than is true in other types of control: the law depends for its instrumentation chiefly upon police, prosecutors, judges, juries, and the support of a favorable public opinion. The law has for its *sanctions* the specifically enumerated punitive measures set up by the state for breach, penalties which are additional to any of the sanctions which society exerts informally against the violator of norms which may overlap with laws. *Crime* is itself simply the breach of the legal norm, a violation within this particular category of social control; the criminal is, of course, the individual who has committed such acts of breach.

Much ink has been spilled on the extent of deterrent efficacy of the criminal law in social control. This is a matter which is not subject to demonstration in any exact and measurable fashion, any more than one can conclusively demonstrate the efficiency of a

a violator of the law: to assume him so would be in derogation of our most basic political and ethical philosophies. In empirical research it would be quite inaccurate, obviously, to study all suspects or defendants as criminals.

moral norm.¹¹ Certainly the degree of success in asserting a control, legal or moral, will vary with the particular norm itself, its instrumentation, the subject individuals, the time, the place, and the sanctions. The efficiency of legal control is sometimes confused by the fact that, in the common overlapping of crimes (particularly those *mala in se*) with moral standards, the norms and sanctions of each may operate in mutual support to produce conformity. Moreover, mere breach of norm is no evidence of the general failure of a social control system, but indication rather of the need for control. Thus the occurrence of theft and homicide does not mean that the law is ineffective, for one cannot tell how frequently such acts might occur in the absence of law and penal sanction. Where such acts are avoided, one may not appraise the relative efficacy of law and mores in prevention. When they occur, one cannot apportion blame, either in the individual case or in general, to failures of the legal and moral systems. The individual in society does undoubtedly conduct himself in reference to legal requirements. Living "beyond the law" has a quality independent of being non-conventional, immoral, sinful. Mr. Justice Holmes has shown that the "bad man of the law"—those who become our criminals—are motivated in part by disrespect for the law or, at the least, are inadequately restrained by its taboos.

From introspection and from objective analysis of criminal histories one can not but accept as axiomatic the thesis that the norms of criminal law and its sanctions do exert some measure of effective control over human behavior; that this control is increased by moral, conventional, and traditional norms; and that the effectiveness of control norms is variable. It seems a fair inference from urban investigations that in our contemporary mass society, the legal system is becoming increasingly important in constraining behavior as primary group

norms and sanctions deteriorate. Criminal law, crime, and the criminal become more significant subjects of sociological inquiry, therefore, as we strive to describe, understand, and control the uniformities and variability in culture.

We consider that the "white collar criminal," the violator of conduct norms, and the anti-social personality are not criminal in any sense meaningful to the social scientist unless he has violated a criminal statute. We cannot know him as such unless he has been properly convicted. He may be a boor, a sinner, a moral leper, or the devil incarnate, but he does not become a criminal through sociological name-calling unless politically constituted authority says he is. It is footless for the sociologist to confuse issues of definition, normation, etiology, sanction, agency and social effects by saying one thing and meaning another.

V

To conclude, we reiterate and defend the contention that crime, as legally defined, is a sociologically significant province of study. The view that it is not appears to be based upon either of two premises: 1. that offenders convicted under the criminal law are not representative of all criminals and 2. that criminal law violation (and, therefore, the criminal himself) is not significant to the sociologist because it is composed of a set of legal, non-sociological categories irrelevant to the understanding of group behavior and/or social control. Through these contentions to invalidate the traditional and legal frame of reference adopted by the criminologist, several considerations, briefly enumerated below, must be met.

1. Convicted criminals as a sample of law violators:

a. Adjudicated offenders represent the closest possible approximation to those who have in fact violated the law, carefully selected by the sieving of the due process of law; no other province of social control attempts to ascertain the breach of norms with such rigor and precision.

b. It is as futile to contend that this group should not be studied on the grounds

¹¹ For a detailed consideration of the efficacy of legal norms, see Jerome Michael and Herbert Wechsler, "A Rationale of the Law of Homicide," 37 *Columbia Law Review* 702, 1261, (1937).

that it is incomplete or non-representative as it would be to maintain that psychology should terminate its description, analysis, diagnosis, and treatment of deviants who cannot be completely representative as selected. Convicted persons are nearly all criminals. They offer large and varied samples of all types; their origins, traits, dynamics of development, and treatment influences can be studied profitably for purposes of description, understanding, and control. To be sure, they are not necessarily representative of all offenders; if characteristics observed among them are imputed to law violators generally, it must be with the qualification implied by the selective processes of discovery and adjudication.

c. Convicted criminals are important as a sociological category, furthermore, in that they have been exposed and respond to the influences of court contact, official punitive treatment, and public stigma as convicts.

2. The relevance of violation of the criminal law:

a. The criminal law establishes substantive norms of behavior, standards more clear cut, specific, and detailed than the norms in any other category of social controls.

b. The behavior prohibited has been considered significantly in derogation of group welfare by deliberative and representative assembly, formally constituted for the purpose of establishing such norms; nowhere else in the field of social control is there directed a comparable rational effort to elaborate standards conforming to the predominant needs, desires, and interests of the community.

c. There are legislative and juridical lags which reduce the social value of the legal

norms; as an important characteristic of law, such lag does not reduce the relevance of law as a province of sociological inquiry. From a detached sociological view, the significant thing is not the absolute goodness or badness of the norms but the fact that these norms do control behavior. The sociologist is interested in the results of such control, the correlates of violation, and in the lags themselves.

d. Upon breach of these legal (and social) norms, the refractory are treated officially in punitive and/or rehabilitative ways, not for being generally anti-social, immoral, unconventional, or bad, but for violation of the specific legal norms of control.

e. Law becomes the peculiarly important and ultimate pressure toward conformity to minimum standards of conduct deemed essential to group welfare as other systems of norms and mechanics of control deteriorate.

f. Criminals, therefore, are a sociologically distinct group of violators of specific legal norms, subjected to official state treatment. They and the non-criminals respond, though differentially of course, to the standards, threats, and correctional devices established in this system of social control.

g. The norms, their violation, the mechanics of dealing with breach constitute major provinces of legal sociology. They are basic to the theoretical framework of sociological criminology.¹²

¹² For other expositions of this view, see articles by Jerome Hall: "Prolegomena to a Science of Criminal Law," 89 *University of Pennsylvania Law Review* 570, (1941); "Criminology and a Modern Penal Code," 27 *Journal of Criminal Law and Criminology* 4, (May-June, 1936); "Criminology," *Twentieth Century Sociology*, pp. 342-65, (1945).

MEASURING DEGREES OF VERIFICATION IN SOCIOLOGICAL WRITINGS*

HORNELL HART
Duke University

IN SEEKING to develop a practicable method of measuring, as accurately as feasible, the degrees of verifiability attained in sociological articles and books, experiments have been made through which readily recognizable and statistically significant indicators have been identified and incorporated in a workable rating technique. The resulting method is based upon the classification of articles in sociological journals, and of randomly selected sentences from those articles. Only signed articles, providing five or more pairs of sample sentences, are used in the basic process. Excluded are book reviews, comments, obituary memorials, rejoinders, committee reports, lists of thesis topics, news items, programs, and all other contents than signed articles. The first step is the classification of such articles into the following types:

A. *Critical Ratio*: articles in which more than half of the space is occupied by the presentation and analysis of statistical data, in connection with which critical ratios, chi squares, or other statistical tests of reliability are used.

B. *Other Statistical*: other articles in which more than half of the space is occupied by statistical tables, maps, charts, or other numerical data, and by their analysis.

C. *Case and Documentary*: articles more than half occupied by a case study or studies, by historical studies, or by annotated quotations treated as data not as authorities, and by the analysis of the foregoing, or of similar materials not presented in the article, but identified as to source. Clear indication must be given that the author gathered his material by comprehensive and impartial coverage of selected cases, organizations, areas, or the like, or by systematic and impartial sampling, rather than by collecting items which happened to attract his interest or to come to his attention. The presentation or citation of comprehensive bibliographies on the

subject is one indicator here. Incidental citations, even though numerous, do not qualify for this category.

D. *Doubtful*: Articles whose classification is uncertain.

E. *"Expert"*: Articles written by authors whose special experiences (such as travel, military or government service, doing other special work, or the like) presumably qualify them to write on the subject discussed, but who present too little verifiable data to be rated A, B, or C.

F. *Other*: If an article has no footnotes, no statistical tables, no textual description of a specific and intensive research project on which the article is based, and no indication of "expertness" of the author, it automatically is classified here. So also are all other articles clearly not belonging in any of the previous classes.

While the foregoing definitions are necessary for reference by the investigator who applies this rating system, the following operational instructions may provide a simpler basis for the one who is starting out to use the method.

First, look for statistical tables. If these occur, note whether they, and the discussion of them, occupy at least half of the article. If so, the article is either A or B.

Second, if this is the case, look for evidence that critical ratios or other, approximately-as-good, methods of testing reliability have been used. If so, the article is A; if not, it is B.

Third, if the article is clearly neither A nor B, look for case histories (usually but not always printed in smaller type than the rest of the article), for comprehensive footnotes, for introductory statements as to methods, and for other evidence as to whether non-statistical factual material, and its discussion, occupy more than half of the article. If such evidence is satisfactory, place the article under C. If both B and C factual data are presented, classify the article under whichever type seems clearly predominant, or place in the D category.

Fourth, if the article is clearly neither A, B, nor C, look for information about the author:

* Manuscript received November 4, 1946.

footnoted to his name, at the end of the article, or in the early paragraphs. If such information indicates clearly that the author had had distinctive experiences peculiarly fitting him to make a contribution on the subject of the article, classify under E.

Fifth, if all of the foregoing evidences are definitely absent, or if they indicate that the article is neither A, B, C, nor E, classify as F.

Sixth, if the evidence is uncertain, classify the article under D.

Independently of the foregoing classification the following process is used for classifying sample sentences. The sentences are selected as follows. First, exclude from this classification all statistical tables, charts, maps, case studies, quotations, titles, subtitles, subheadings, abstracts, outlines, and questionnaires. Otherwise, on pages set in one column, take the most important clause in the first complete sentence on each page, and the most important clause in the first complete sentence whose major portion lies below the middle of each page. When two or more clauses seem equally important, the first of them is to be taken. On two-column pages, the most important clause in the first complete sentence in each column is taken. Where the end of one article and the beginning of another are on the same page, samples of both are taken. If the matter on a given page is not sufficient for classification under the foregoing rules, that page is omitted from consideration.

The sentences thus selected are classified into the following groups:

a. *Refined statistical*: sentences containing any of the following statistical symbols, or their equivalents: C. R., σ , r , ρ , or chi square, or discussing concepts immediately related to them, or discussing index numbers, test scores, curve fitting, skewness, factor analysis, or other equally or more complex statistical procedures. Sentences are *not* included here merely because they mention percentages, means, medians, or the like.

b. *Other specific factual*: any other sentence reporting a specific fact, relating to a specified time, place, and unit, the source of which fact is given verifiably by specific reference to a published document, an available manuscript, or other source from which verification is prac-

ticable. Footnotes (if factual) and bibliographical items are also classified here.

c. *Factual generalization*: any generalization, comparison, or prediction (including tentative interpretations) clearly based on facts of the b type, or on data specified in paragraphs A or B, but not belonging in a.

d. *Method*: any explanation of how such facts, presented in the article in question, were collected, analyzed, or presented, excepting sentences which belong under a. In rating books, index items are classified here.

e. *Problem*: the statement of any problem or hypothesis being investigated in the article or study, or emerging out of them as a question for further study.

f. *Doubtful*: any sentence not clearly belonging in any of the other categories, including all non-valuational statements of apparently specific alleged fact which do not come under a or b, and all generalizations offered on the basis of one or a few real examples or illustrations, not evidentially adequate to support the conclusions offered, or based on alleged investigations not verifiably described.

g. *Non-factual generalization*: any generalization, comparison, or prediction, not clearly belonging in the a, c, d, or e categories. Include here all non-factual, non-valuational generalizations quoted or cited with expressed or implied endorsement, and all imaginary or unspecific illustrations and deductions therefrom.

h. *Bold value judgment*: any statement or clear implication (not clearly belonging in the a, b, c, or e categories) in which the author praises, blames, denounces, urges, stresses, exhorts, or advises; any use of satire or of any terms implying any emotional pressure by the author except dispassionate search for truth. (Do not include here such statements as, "It is interesting to note. . .")

APPLICATION OF CLASSIFICATIONS TO SAMPLE ARTICLES

A basic step in the preliminary investigation was to test the hypothesis that a significant degree of association would be found between types of articles and types of sentences, as defined above. To test this hypothesis, sample sentences were selected by the foregoing rules from a "basic sample" consisting of four numbers from the *American Sociological Review* (February, April, June, and August, 1945, Vol. 10, pp. 1-523)

and two numbers each from *The American Journal of Sociology* (July and September, 1945, Vol. 51, pp. 1-141), the *Journal of Educational Sociology* (September and October, 1943, Vol. 17, pp. 1-113), *Rural Sociology* (March and June, 1944, Vol. 9, pp. 1-177), *Social Forces* (October and December, 1943, Vol. 23, pp. 1-212), *Social Science* (January and April, 1944, Vol. 19, pp.

sample, making a "supplementary A" group of 779 sentences, or a grand total of 3,151 sentences. The distribution of these, by types of sentence and of article, is shown in Table 1.

The hypothesis that association exists between types of articles and types of sentences is confirmed by the distribution in Table 1. The value of chi square is 1360,

TABLE 1. SENTENCES OF BASIC SAMPLE AND OF SUPPLEMENTARY "A" GROUP, DISTRIBUTED ACCORDING TO TYPES OF SENTENCES AND OF ARTICLES, WITH CALCULATION OF SCORING WEIGHTS

Line No.	Types of Articles	Sentences	Percentage Distribution								Total
			a	b	c	d	e	f	g	h	
1.	A: critical ratio	965	17.5	5.8	30.6	31.8	7.8	1.5	3.9	1.1	100.0
2	supplementary basic	779	18.8	5.9	29.5	32.2	8.0	1.4	3.4	.8	100.0
3		186	12.5	5.4	34.4	30.1	7.0	1.6	6.4	2.7	100.0
4	B: other statistical	372	1.1	5.9	46.0	25.8	7.2	2.9	8.9	2.2	100.0
5	C: case and documentary	417	.0	14.1	33.4	9.6	4.6	12.2	19.4	6.7	100.0
6	D: doubtful	336	.6	9.2	7.7	6.2	2.4	16.1	44.4	13.4	100.0
7	E: "expert"	390	.0	8.5	5.1	4.6	1.8	12.6	47.6	19.8	100.0
8	F: other	671	.0	2.2	.6	3.4	3.1	9.0	52.0	29.7	100.0
9	Total: Basic sample	2372	1.2	7.2	17.9	10.7	4.0	9.6	34.1	15.3	100.0
10	Grand	3151	5.6	6.8	20.8	16.0	5.0	7.6	26.5	11.7	100.0
11	A, B, and C*	1754	6.2	8.6	36.7	22.4	6.5	5.5	10.8	3.3	100.0
12	Line 11 minus line 8	1083	6.2	6.4	36.1	19.0	3.4	-3.5	-41.2	-26.4	0.0
13	Line 11 plus line 8	2425	6.2	10.8	37.3	25.8	9.8	14.5	62.8	33.0	200.0
14	Line 12 + line 13	1.000	.593	.969	.737	.354	-.241	-.656	+.800		
15	Line 14 plus .800	1.800	1.393	1.769	1.537	1.154	.559	.144	.000		
16	Proportional weights		4	3	4	4	4	1	0	0	

* The three types are given equal weight in determining the percentage distribution.

1-93), *Sociometry* (February and May, 1944, Vol. 7, pp. 1-244) and *Sociology and Social Research* (September-October and November-December, 1943, Vol. 28, pp. 1-143). From these 18 numbers 2,372 sample sentences were selected by the method indicated above. In addition to the basic sample, all other articles (including those with less than five pairs of sample sentences) were classified from all of the above eight sociological periodicals, for the entire period of July 1943 to June 1946. This will be called the "three-year sample." Sample sentences were classified from all A-type articles not previously analyzed from this three-year

with 35 degrees of freedom. The critical ratio for such a chi square is 43.5, indicating an overwhelming probability that the distribution in Table 1 is not due to mere random scatter. The value of C is .604.

THE VERIFIABILITY SCORES OF THE BASIC SAMPLE OF ARTICLES

Having demonstrated that the types of sentences defined above are significantly associated with the defined types of articles, the next step is to devise a scoring system for calculating the most reliable index of the degree of scientific verifiability attained in a given article. The steps in this process are

shown in lines 12 to 16 of Table 1. The only basic assumption involved here is that articles of the A, B, and C types represent higher degrees of scientific verification than articles of the F type. The procedures of lines 12 to 16 simply measure the extent to which the presence or absence of the various types of sentences serve to differentiate between the more scientific and the less scientific types of articles.

in Table 1 and those actually used is that for type-g sentences a weight of 1 was employed instead of 0. The improved statistical process shown in Table 1 indicates that bald value judgments are so slightly more indicative of poor scientific quality than are unsupported generalizations that a zero rating for both is preferable.¹ This makes possible a simplification of instructions for rating sentences, throwing the g- and h-type

TABLE 2. MEAN SCORES DERIVED FROM SAMPLE SENTENCES, BY TYPES OF ARTICLES AND BY PERIODICALS, FOR THE BASIC SAMPLE OF ARTICLES

Periodical #	Type of Article													
	A: Critical Ratio*		B: Other Statistical		C: Case and Documentary		D: Doubtful		E: "Expert"		F: Other		All Types	
	No.	Mean Score	No.	Mean Score	No.	Mean Score	No.	Mean Score	No.	Mean Score	No.	Mean Score	No.	Mean Score
ASR	5	43.3	10	34.9	9	30.5	9	22.1	5	21.3	11	10.5	49	26.6
RS	1	44.5	3	37.4	2	34.0	1	14.0	4	14.9	3	11.3	14	23.7
S	2	40.5	1	38.5	3	23.0	2	27.3	1	12.0	3	7.2	12	23.0
AJS	1	48.0	2	37.2	1	34.0	3	12.2	2	18.2	2	8.5	11	22.4
SF	0	—	5	36.3	8	23.4	2	12.0	6	9.9	6	9.2	27	18.8
SSR	0	—	3	33.7	1	23.0	3	14.2	0	—	8	9.5	15	16.2
JES	0	—	0	—	0	—	1	6.5	8	13.0	6	8.8	15	10.9
SS	0	—	0	—	1	14.5	0	—	0	—	6	8.1	7	9.0
Total	9	43.3	24	35.7	25	26.8	21	18.0	26	14.5	45	9.3	150	41.2

* Of the critical-ratio group (type A) only the articles of the basic sample are included here.

The abbreviations refer to the following periodicals: AJS: *American Journal of Sociology*; ASR: *American Sociological Review*; JES: *Journal of Educational Sociology*; RS: *Rural Sociology*; S: *Sociometry*; SF: *Social Forces*; SS: *Social Science*; SSR: *Sociology and Social Research*.

The weights in line 16 differ in three respects from those actually employed in determining the scores reported later in this article. The first of these is that the weight used for a-type sentences was 10 instead of 4. This is simply in recognition of the fact that type-a sentences are the only ones which differentiate decisively between the A-type articles on the one hand and the B- and C-type articles on the other. The extra weighting is based upon one additional assumption—namely, that articles which apply the theory of probability provide thereby a higher degree of scientific verification than articles which do not.

A second difference between the weights

sentences into one category, and giving no attention to the problem of discriminating between them.

The third difference is that a weight of 2 was used for type-f sentences instead of the indicated weight of 1. This, and the difference discussed in the preceding paragraph, do not affect the results to any important degree.

The following rule was the one actually used for scoring the articles on the basis of

¹ A report on a preliminary research to determine the relation of value judgments to scientific verifiability will be found in "Factuality, and the Discussion of Values," by the present writer, accepted in October, 1946, for publication in *Social Forces*.

sample sentences. By altering the weight of f-type sentences from 2 to 1, and omitting all weights for the g-type, the rule can be employed for further rating of either articles or books: MULTIPLY THE NUMBER OF a-TYPE SENTENCES BY 10, THE NUMBER OF c-TYPE PLUS d-TYPE BY 4, THE NUMBER OF b-TYPE PLUS e-TYPE BY 3, THE NUMBER OF f-TYPE BY 2, AND THE NUMBER OF g-TYPE BY 1. ADD THE PRODUCTS, MULTIPLY BY 10, AND DIVIDE BY THE TOTAL NUMBER OF SAMPLE SENTENCES, INCLUDING g-, AND h-TYPES. In order to calculate chance-half reliabilities, this rule was applied separately to the sentences coming first on each page and those coming second; to obtain the sentence score for a given article these two scores were averaged.

The mean scores obtained by this method, for the various types of articles, and for each of the eight journals analyzed, are shown in Table 2.

Two independent methods of determining the scientific verifiability of sociological articles have now been presented. The first consists in classifying the articles into types, on the basis of readily ascertainable characteristics of each article as a whole; the second consists in classifying sample sentences into categories, and scoring in accordance with the rule given at the beginning of the present section. The method of classifying articles as a whole can now be reduced to a quantitative basis by assigning to each article the mean score of its particular type, as ascertained in Table 2. The operational rule for doing this is as follows: FOR ARTICLES RATED BY SAMPLE SENTENCES, ASSIGN TO EACH ITS SAMPLE-SENTENCE SCORE, PLUS THE FOLLOWING VALUES FOR THE RESPECTIVE TYPES: A, 47;² B, 36; C, 27; E, 15; F, 9. For D ARTICLES, MULTIPLY SAMPLE SENTENCE SCORES BY 2. VALUES OBTAINED IN

THIS WAY WILL BE REFERRED TO AS "FULL SCORES." FOR ARTICLES NOT RATED BY SAMPLE SENTENCES ASSIGN THE FOLLOWING SCORES TO THE RESPECTIVE TYPES: A, 94; B, 72; C, 54; D, 36; E, 30; F, 18. VALUES OBTAINED IN THIS WAY WILL BE REFERRED TO AS "DOUBLED-TYPE SCORES."

RELIABILITIES

The reliabilities of sample-sentence scores, doubled-type scores, and full scores are as indicated by the data in Table 3.

For scoring the F group of articles, the use of sample sentences adds nothing to the reliability of the scores. The standard deviation of sample-sentence scores in this group is so small that the chance-half correlation is negligible. For the "D: doubtful" group, on the other hand, the standard deviation within the type is so large that most of the reliability comes from the sample-sentence scores. The total reliability of scores for the A-type articles, as shown in column (12) is lower than that of any other type, due to the fact that the heavy weighting of the small number of a-type sentences makes the fluctuation of scores rather erratic.

The net reliability of the combined scoring methods, for all types of articles combined, is summarized by the final figure in column (12) of Table 3. The full scores, based on both type of article and on sample sentences, account for 94 per cent of the variance in the scores which would be obtained from a perfectly reliable measure of that which these scores measure. These scoring processes have thus proved highly reliable.

THE VALIDITY OF VERIFIABILITY SCORES ON SOCIOLOGICAL BOOKS

To test validity the best available procedure seemed to be to apply the same scoring method (adapted as necessary) to an impartial sample of sociological books, and to compare these scores with ratings by the most competent judges obtainable. For this purpose a list was made of every book indexed in *The Book Review Digest*

²The A-type score is based upon the entire group of A articles, including the supplementary sample. See Table 3.

for 1944, 1945, and 1946 (August issue) under the headings *Anthropology, Crime and criminals, Divorce, Family, Juvenile delinquency, Population, Race, Race problems, Social problems, Social psychology, Social*

can Journal of Sociology. Finally, only those available in the Duke University Library at the date of the study were included. The 29 books qualifying under these rules are those listed in Table 4.

TABLE 3. RELIABILITY DATA FOR THREE TYPES OF VERIFIABILITY SCORES OBTAINED FROM THE BASIC AND SUPPLEMENTARY SAMPLES OF SOCIOLOGICAL ARTICLES

Articles		Sample-Sentence Scores							Percent of Remaining Variance Accounted for by		
Type	No.	Mean	S.D.**	Correlation Ratio	Z σ_1	Chance-Half Correlation σ_2	Z σ_2	Correlation Ult. Score	Column (5)	Column (9)	Column Scores
	(2)	(3)	(4)	(5)***	(6)	(7)**	(8)#	(9)##	(10)†	(11)††	(12)*†
A: critical ratio*.....	50	47.3°	8.86	.714	6.1	.433	3.2	.777	.51	.60	.81
B: other statistical.....	24	35.7	7.64	.796	5.0	.748	4.6	.923	.64	.85	.95
C: case and documentary.....	25	26.8°	6.51	.857	6.0	.636	3.7	.882	.74	.78	.94
D: doubtful.....	21	18.0	9.35	.674	3.5	.794	4.6	.942	.45	.89	.94
E: "expert".....	26	14.5	7.51	.804	5.3	.777	4.9	.935	.65	.87	.96
F: other.....	45	9.3	3.08	.970	13.6	†††	.8	†††	.94	—	.94
Entire sample*.....	191	20.6	12.69	—	—	.892*#	17.3	.972*#	—	—	.94

* Includes supplementary sample.

** Corrected for size of sample.

*** Column (5) presents correlation ratios, based on the formula:

$$\bar{r}^2 = \left(1 - \frac{S_y^2}{\sigma_y^2} \right) \frac{N-1}{N-m} - \frac{m-1}{N-m}$$

See Croxton, Frederick E., and Cowden, Dudley J., *Applied General Statistics*, 1940, p. 683.

This column gives the correlation of the combined chance-half scores of each article with the score which would have been obtained by an indefinitely long continuation of the scoring process, if such could have been carried out without altering the conditions of the actual investigation. The formula is:

$$r(\frac{1}{2} + \frac{1}{2})_{\infty} = \sqrt{\frac{2r_{\frac{1}{2}}^2}{1 + r_{\frac{1}{2}}^2}}$$

The algebraic demonstration of this formula has been omitted for lack of space.

† Column (10) is column (5) squared.

†† Column (11) is column (9) squared.

*† Column (12) is column (10) plus [column (11) times one minus column (10)].

° The difference between the means of types A and C is 20.5 with a critical ratio of 6.6. This is consistent with the findings of Theodore Sarbins "A Contribution to the Study of Actuarial and Individual Methods of Prediction," *American Journal of Sociology*, Vol. 48 (1943), p. 593.

*# Based on full scores.

††† The crude chance-half correlation for the F type of article is .127; when corrected for size of sample this becomes imaginary. The low critical ratio in column (8) of this line confirms this conclusion.

progress, Social surveys, Social work, Society, primitive, Sociology, and Sociology, rural. Of the books listed under these heads, only those were selected which were reviewed in the *American Sociological Review* or *The Ameri-*

As a criterion on which to base validity, judgments were requested from 252 members of the American Sociological Society who had published articles in any of the eight sociological journals listed in Table 2,

from July 1, 1943, to June 30, 1946. The list of 29 books, as given in Table 4, was enclosed to each of the judges in a letter mailed out during the last week of September, 1946, with a return postcard on which they were invited to list the three, four, or five books whose methods seemed to them to be least scientific, and the three, four, or five whose methods seemed most scientific.

letters, this is a fairly high percentage.³ A 100-per cent return would not have been desirable in the present inquiry, even if it could have been attained, since, in addition to the 21 who so stated, a considerable proportion of the sociological authors to whom the letter was sent are presumably too unfamiliar with the books listed to render a discriminating judgment. Many who are

TABLE 4. AUTHORS, TITLES, PUBLISHERS, AND YEARS OF PUBLICATION OF 29 SOCIOLOGICAL BOOKS, IMPARTIALLY SELECTED BY THE METHOD DESCRIBED IN THE TEXT

ABRAHAMSEN, DAVID, <i>Crime and the Human Mind</i> , Columbia Univ. Press, 1944.
CHANDLER, ALBERT R., <i>Rosenberg's Nazi Myth</i> , Cornell Univ. Press, 1945.
CLAYTON, ALFRED S., <i>Emergent Mind and Education</i> , Teacher's College, 1943.
DU BOIS, WILLIAM EDWARD BURGHARDT, <i>Color and Democracy</i> , Harcourt, Brace, 1945.
ELMER, MANUEL C., <i>Sociology of the Family</i> , Ginn & Co., 1945.
GLUECK, SHELDON and ELEANOR, <i>Criminal Careers in Retrospect</i> , Commonwealth Fund, 1943.
GOLDSTEIN, SIDNEY E., <i>Marriage and Family Counseling</i> , McGraw-Hill, 1945.
GREENWOOD, ERNEST, <i>Experimental Sociology</i> , King's Crown Press, 1945.
GROVES, ERNEST R., <i>Conserving Marriage and the Family</i> , Macmillan, 1944.
HOFSTADTER, RICHARD, <i>Social Darwinism in American Thought, 1860-1915</i> , Univ. of Pennsylvania Press, 1944.
HUSZAR, GEORGE BERNARD DE, <i>Practical Applications of Democracy</i> , Harper, 1945.
JENNINGS, HELEN HALL, <i>Leadership and Isolation</i> , Longman's Green, 1943.
KELSEN, HANS, <i>Society and Nature</i> , Univ. of Chicago Press, 1943.
KROEBER, ALFRED L., <i>Configurations of Culture Growth</i> , Univ. of California Press, 1944.
LANDIS, PAUL HENRY, <i>Population Problems</i> , American Book, 1943.
LANDRY, STUART O. <i>The Cult of Equality</i> , Pelican, 1945.
LEIGHTON, ALEXANDER H., <i>The Governing of Men</i> , Princeton Univ. Press, 1945.
MALINOWSKI, BRONISLAW, <i>Scientific Theory of Culture</i> , Univ. of North Carolina Press, 1944.
MANNHEIM, KARL, <i>Diagnosis of Our Time</i> , Oxford Univ. Press, 1944.
MARTIN, ALFRED WILHELM VON, <i>Sociology of the Renaissance</i> , Oxford Univ. Press, 1944.
MYRDAL, GUNNAR, <i>An American Dilemma</i> , Harper, 1944.
REIK, THEODOR, <i>Unknown Murderer</i> , Prentice-Hall, 1945.
RICHARDSON, HENRY BARBER, <i>Patients Have Families</i> , Commonwealth Fund, 1945.
SOROKIN, PITIRIM A., <i>Socio-Cultural Causality, Space, Time</i> , Duke Univ. Press, 1943.
STURZO, LUIGI, <i>Inner Laws of Society</i> , Kenedy, 1944.
THOMPSON, WARREN S., <i>Plenty of People</i> , Cattell, 1944.
WACH, JOACHIM, <i>Sociology of Religion</i> , Univ. of Chicago Press, 1944.
WAITE, JOHN BARKER, <i>The Prevention of Repeated Crime</i> , Univ. of Michigan Press, 1943.
YANG, MOU-CH'UN, <i>A Chinese Village</i> , Columbia Univ. Press, 1945.

Through November 9, 1946, usable returns had been received from 76 of the judges. Of the remainder, 21 replied that they were not sufficiently acquainted with the books to express an opinion, 4 declined because of disapproval of the method, 9 letters were returned because of change of address and the remainder had not yet replied.

The 97 definite replies, out of 252 inquiries sent out, constitute a 38.5 per cent return on a single solicitation. As compared with usual proportions of replies to circular

thus uninformed would probably hesitate to state that, fact in writing, and therefore failed to reply. Moreover, analysis of the returns which were sent in shows such internal consistency that the validation is confirmed by a variety of different tests.

The number of times that each book was rated as "least scientific" and "most scien-

³ Croxton and Cowden, *op. cit.*, p. 34, say: "A large proportion of persons failed to reply to mail inquiry. . . ." Cf. George A. Lundberg, *Social Research*, 1942, pp. 203-205.

tific" in method, by the judges who sent in usable returns, is indicated in Table 5, together with the verification scores determined and recorded on September 14—i.e. a week before opinions of the judges were solicited. From Table 5 it will be noted that the weighted mean verification score of the books which these judges regarded as "most scientific" was 51.04, while the weighted mean score of the books rated as "least

Study of Table 6 shows that whether one confines his attention to the first 39 replies, or to the next 28, or to the final 9, the conclusions are essentially the same as if the entire 76 replies are used. For each subsample the difference between the previously ascertained verification scores of books rated as "most scientific" and as "least scientific" in method is over five times the standard deviation of that difference. Even if the last

TABLE 5. BOOKS OF TABLE 4, ARRANGED IN ORDER OF PREVIOUSLY ASCERTAINED VERIFICATION SCORES, SHOWING FREQUENCIES WITH WHICH EACH BOOK WAS RATED AS "LEAST SCIENTIFIC" OR "MOST SCIENTIFIC" IN METHOD BY 76 AUTHORS OF SOCIOLOGICAL ARTICLES WHO HAD NOT BEEN INFORMED OF THE SCORES

Author	Verification Score	Frequency of Rating as		Author	Verification Score	Frequency of Rating as	
		"Least Scientific"	"Most Scientific"			"Least Scientific"	"Most Scientific"
Jennings.....	89	1	18	Malinowski.....	24	4	23
Glueck.....	71	0	51	Abrahamsen.....	24	10	0
Landis.....	65	3	21	Du Bois.....	18	23	0
Leighton.....	59	5	9	Groves.....	17	28	3
Richardson.....	58	3	7	Sorokin.....	15	31	7
Kelsen.....	57	15	1	Huszar.....	13	6	1
Kroeber.....	57	4	25	Mannheim.....	13	15	5
Greenwood.....	56	0	33	Sturzo.....	12	12	0
Yang.....	56	1	13	Martin.....	9	1	3
Clayton.....	55	7	1	Goldstein.....	3	11	0
Waite.....	51	2	3	Total.....		241	314
Chandler.....	50	7	0	Mean scores.....		27.43	51.04
Wach.....	46	3	6				
Myrdal.....	41	4	50	Difference			
Hofstadter.....	40	6	3	($M_M - N_L$).....		23.61	
Elmer.....	33	16	7	$\sigma_{M_M - M_L}$		1.88	
Reik.....	28	13	1	Critical ratio.....		12.6	
Thompson.....	28	4	23				
Landry.....	26	6	0				

scientific" was 27.43. The difference between the two is 23.61; the standard deviation of the difference is 1.88 and the critical ratio is 12.6. In a perfectly normal frequency distribution a critical ratio of 12.5 is exceeded, in one direction or the other about once in 10^{35} times (i.e., once per 10 followed by 35 zeros).

Some further indications as to the reliability of the returns from these 76 judges, and as to the degree of their association with the verification scores, may be gleaned from the supplementary statistical analysis presented in Table 6.

nine replies had been the only ones available, the validation would have been such as would occur by chance less than once in 10,000,000 times. On the basis of the foregoing analyses, it seems safe to conclude that the verification scores which were obtained in advance of the postcard judgments did validly (though not perfectly) reflect whatever it is that differentiates sociological writings designated as "most scientific" from those designated as "least scientific" by such sociologists as these 76 judges.

It should be noted that the judgments obtained cannot be assumed to measure scien-

tific quality perfectly, and that therefore even an ideal measure of scientific reliability would not be expected to correlate perfectly with the pooled judgments. Some of the differences between the pooled ratings and the scores are, however, so striking and so consistent that brief interpretive comments seem in order. In particular, Kelsen's *Society and Nature*; (a *Sociological Inquiry*) obtained a verification score of 57, but is rated as "least scientific" by 15 judges, and "most scientific" by only 1; while Malinowski's *A Scientific Theory of Culture (and other Essays)* received a verification score of only 24, but was rated as "least scientific" by only 4 judges and as "most scientific" by 23.

suggests armchair philosophizing. Moreover, in addition to his numerous books in German and French, Kelsen had previously published in English the following titles: *The Legal Process and International Order*, 1935; *Law and Peace in International Relations*, 1942; *Peace through Law*, 1944; and *General Theory of Law and State*, translation published in 1945. Such books might be expected to give Kelsen a reputation as a social philosopher rather than as a scientist. Again, it seems likely that relatively few of the judges consulted Kelsen's *Society and Nature* thoroughly enough to observe that it is a carefully annotated scholarly study, consisting of factual statements about the

TABLE 6. VERIFICATION-SCORE CONSTANTS FOR THREE CONSECUTIVE SUBDIVISIONS OF THE RATINGS SUMMARIZED IN TABLE 5

Description of Sample	Number of Ratings as		Mean Verification Scores			S.D. of Difference	Critical Ratio
	Least Scientific	Most Scientific	Least Scientific	Most Scientific	Difference		
First 39 replies*.....	124	159	28.48	50.17	21.69	2.63	8.3
Next 28 replies.....	82	112	26.45	51.37	24.92	3.18	7.8
Last 9 replies.....	36	39	25.92	53.59	27.67	5.14	5.4
All 76 replies.....	242	310	27.43	51.04	23.61	1.88	12.6

* Excluding those who declined to submit ratings.

(The portions of the titles in parentheses were omitted from the mimeographed list sent to the judges.)

Fairly obvious hypotheses suggest themselves as explanations of these two exceptions to the unquestionable association between verification scores and pooled ratings for the list as a whole. First, Malinowski's book has the word "scientific" in the title as quoted; he also had established a reputation as a field anthropologist. Most of the judges presumably did not examine the book itself carefully enough to observe that it is almost completely devoid of detailed footnotes and of citations of data, and that it consists almost entirely of sweeping generalizations and bald value judgments, supported only by its author's reputation.

Kelsen's book has the kind of title which

writings which he was analyzing, and of generalizations specifically based on those facts.

In the absence heretofore of any generally accepted specifications defining scientific method in sociological writings, and in view of the limited leisure which sociologists have to answer inquiries of the kind used in this study, it would be inconceivable that any broad group of such pooled judgments as these would correlate perfectly with any scores based on objective analyses. Systematic biases of the kinds suggested in the hypotheses just presented would normally be expected. The only validation which would seem to be possible through comparison of the results of systematic scoring with such ratings as it is practicable to obtain from any representative group of sociologists,

is the establishment of clear-cut, unmistakable but incomplete association between the conclusions reached by these two methods. Such association has been proven by the results summarized in this section of the present article.

THE THREE-YEAR SAMPLE OF SOCIOLOGICAL ARTICLES

Classification of articles into types can be done considerably more rapidly than

The rank order of the periodicals in Table 7 is slightly different from that in Table 2. This is due to the fact that Table 2 represents only the basic sample of 150 articles, while Table 7 is based on 1076. The difference between the mean scores of *Rural Sociology* and the *American Sociological Review* (based on the data of Tables 3 and 7) is 3.4, with a critical ratio of 2.24, which would occur by chance about 25 times per 1000. The difference between the scores of

TABLE 7. MEAN SCORES, BASED ON TYPES OF ARTICLES, FOR THREE-YEAR SAMPLE, BY PERIODICALS

Periodical	Number of Articles	Percentage Distribution							Mean Score
		A	B	C	D	E	F	Total	
RS.....	69	11.6	23.2	21.7	11.6	13.0	18.9	100.0	51.1
ASR.....	198	10.6	22.2	14.7	17.7	10.6	24.2	100.0	47.7
SF*.....	169	3.0	18.9	29.6	13.0	13.6	21.9	100.0	45.1
AJS.....	142	7.7	14.1	20.4	14.1	14.8	28.9	100.0	43.4
S#.....	83	9.6	10.9	19.3	18.1	8.4	33.7	100.0	42.0
SSR.....	113	.9	12.4	15.9	14.2	12.4	44.2	100.0	35.1
JES**.....	181	.0	3.9	11.1	8.8	39.8	36.4	100.0	30.4
SS.....	121	.0	.0	3.3	3.3	43.0	50.4	100.0	24.9
Total.....	1,076	5.0	13.2	16.8	12.6	20.4	32.0	100.0	39.7

* Contents for October, 1945, estimated.

Same for July to December, 1945.

** Same for Vol. 18.

One notable omission from the present study should be mentioned. The (British) *Sociological Review* has not been included in the investigations summarized in Table 7. This deficiency has been met by an analysis of 35 articles from that *Review*, for the period January, 1941 to October, 1944, by Marian Besant, Helen V. Matthews, and Hornell Hart. An article embodying the results of this supplementary study is being offered to the *Sociological Review* for possible publication.

A second supplementary study covers three issues each from *The Journal of Abnormal and Social Psychology*, the *American Sociological Review*, and the *Political Science Quarterly*, during 1946. That study was made cooperatively by ten students, under Dr. Hart's direction. It is hoped that it also may be published soon.

scoring by means of sample sentences. In order to supplement the as yet inadequate sample of 191 articles analyzed in Table 3, a study has been made of all the articles published in all of the eight sociological journals during the three-year period from July 1, 1943 to June 30, 1946.⁴ The distribution of the 1076 articles published during this period is shown, by type of article and by periodical, in Table 7.

⁴With the exception of a few numbers not obtainable at the time of this study, for which estimates were substituted.

the *American Sociological Review* and *Social Science* is statistically significant in both tables.

For the purpose of estimating the relative scientific quality of a given article, as compared with the three-year average, it may be helpful to know the fiduciary limits of the distribution of full scores in the eight sociological journals combined. Assuming that the full scores of each type of article are distributed similarly to the distribution in the basic and supplementary samples, the chances are less than one in 100 that less

than 71 per cent or more than 77 per cent of sociological articles score over 20; or that less than 47 per cent or more than 53 per cent score over 30; or that less than 32 per cent or more than 38 per cent score over 50. For the upper end of the distribution, the best present smoothed estimates of the cumulative percentages of articles rating above the indicated scores are as follows:

Score	Estimated percentage scoring higher
100	1.0
95	2.0
90	3.6
85	4.5
80	8.0
75	10.5
70	12.0
65	17.4
60	21.5
55	26.0

SOME CRITICISMS ALREADY RECEIVED

Several critics of this manuscript have suggested that other qualities besides scientific method are important in sociological writings. Dr. Sorokin, in returning his postcard vote, added the note: "Your question does not take into consideration a) the importance and difficulty of the problems studied . . . b) the essential insight and understanding of the phenomenon. . . ." Robert E. L. Faris wrote: "I respect some . . . even though not particularly 'scientific'" Bruno Lasker noted: "Such books as (. . .) are not very rigid in method, but nothing should be done to discourage such admirable interpretive studies!"

Let it be said in reply that the present

study does not attempt to measure originality, nor the value of contributions to sociological knowledge, aside from the rigor of the methods used to establish the objective truth and the predictive reliability of conclusions presented. The author of the present article assumes that all published discussion of sociological problems has some actual or potential value, in that it puts on record the efforts of social thinkers to formulate such problems, offers at least tentative concepts, and makes available to subsequent investigators evidence of successes and failures in social thinking. The rating methods set forth in this paper do not, therefore, constitute any disparagement of anyone's writings. The scores are all positive. The purpose of these ratings is simply to provide verifiable measurements of the varying degrees to which various sociological articles present scientifically verifiable results.

A second type of criticism suggests not only that other factors need to be taken into account in addition to scientific method, but that attempts to measure degrees of verification may be positively harmful at the present time. Dr. Robert C. Angell writes: "A great many of us are of the opinion that we need more thorough qualitative analysis prior to further elaboration on the quantitative side." H. C. Brearley observed on his postcard reply: "Being 'scientific' may not be any great virtue." The present writer agrees that quantitative methods need always to be subjected to rigorous criticism in the light of common sense and of qualitative meaning, but he believes that when thus criticized, quantitative methods are of vital importance in the development of sociology.

THE SOCIOLOGY CURRICULUM AND TEACHER TRAINING*

JUDSON T. LANDIS
Michigan State College

THERE has been a sharp increase in the teaching of sociology, social problems, problems of American democracy and related courses in the high schools of the nation during the last fifteen years. In the

past the social science offerings in high school were limited largely to courses in ancient, medieval, modern and American history, but many high schools are now requiring students to take a major in the social studies. With the current change the ancient, medieval and modern history is telescoped into a course

* Manuscript received November 18, 1946.

in world history. In the place of the other history courses are courses in economics and sociology. This expansion of the social science curriculum in high school puts upon the teacher training institutions the obligation to keep up with these curriculum changes and to qualify social science teachers to teach all of the social studies.

In a study of the teaching of sociology in Illinois high schools the writer found that of the sociology teachers in the state only two per cent had majored in sociology. More had majored in English, science, and education than sociology. However, more of the sociology teachers were drawn from related fields; two out of three having majored in history

and type of courses being offered in the colleges a study was made of all courses listed in the 1944-45 annual bulletins of the 162 teachers colleges recognized by the American Association of Teachers Colleges. All courses taught were noted and an analysis was made by sections of the country in which the colleges are located. One hundred and thirteen different course titles were recorded in the 162 teachers college bulletins. The list of titles would be much longer than this if no attempt were made to combine those of a similar content.

One of the first things one observes in studying college bulletins is the great difference in the number of courses offered in

TABLE 1. NUMBER OF DIFFERENT COURSES AND FREQUENCY OF OFFERING SOCIOLOGY COURSES IN THE 162 TEACHERS COLLEGES, BY REGIONS

	Middle Atlantic and New England	Mid- west	North- west	South- west	South Atlantic and South	All Regions
Number of different course titles listed.....	21	88	7	46	39	113
Number of times courses were listed.....	72	522	23	215	190	1022
Number of colleges in each region..	38	61	8	22	33	162
Average number of courses offered per school.....	1.9	8.6	2.9	9.8	5.6	6.3

with additional courses in civics and economics.¹ Although half of these teachers had the masters degree, two out of three had less than ten semester hours in sociology and one in seven had no training at all in the field.

This study of the preparation of high school teachers caused the writer to become interested in the sociology curriculum in the colleges which prepare teachers to teach in high schools. Just what are the teachers colleges doing to educate their students in sociology? Are sociologists in the colleges aware of the fact that the high schools are requiring their teachers to teach sociology whether or not these teachers are adequately prepared?

In order to get some idea as to the num-

ber and type of courses being offered in the colleges a study was made of all courses listed in the 1944-45 annual bulletins of the 162 teachers colleges recognized by the American Association of Teachers Colleges. All courses taught were noted and an analysis was made by sections of the country in which the colleges are located. One hundred and thirteen different course titles were recorded in the 162 teachers college bulletins. The list of titles would be much longer than this if no attempt were made to combine those of a similar content.

One of the first things one observes in studying college bulletins is the great difference in the number of courses offered in the five regions of the country. The Middle Atlantic and New England states offer very little sociology when compared with other regions of the country. The Northwest region comes next above the Middle Atlantic and New England region. The Southwest leads in offerings. The Midwest ranks second highest. The South Atlantic and South ranks third in regional offerings. (See Table 1.)

In the majority of cases schools in the Midwest, Southwest, and South Atlantic and South regions have a separate Department of Sociology. If there is no sociology department the work is offered in the social science department as it is in the Middle Atlantic and New England, and the Northwest regions.

A look at the titles of the courses offered shows that the one listed most frequently is Principles of Sociology. The next most common course in four regions is Social Prob-

¹ Landis, Judson T. "Sociology in Illinois High Schools," *The Social Studies*, December, 1942.

TABLE 11. TITLES OF SOCIOLOGY COURSES LISTED IN THE COLLEGE BULLETINS OF THE 162 TEACHERS COLLEGES, BY REGIONS AND FREQUENCY

Titles	Middle Atlantic and New England	Mid- west	North- west	South- west	South Atlantic South	Total
(Number of Colleges).....	(38)	(61)	(8)	(22)	(33)	(162)
Principles of Sociology.....	32	66	11	25	35	169
(Introduction to)						
Social Problems.....	7	47	4	21	16	95
Marriage & The Family.....	5	39	—	16	23	83
Rural Sociology.....	4	41	2	14	21	82
Criminology.....	—	28	—	12	12	52
Anthropology.....	3	21	3	4	7	38
Race Relations (Minorities).....	—	18	—	10	9	37
Urban Sociology.....	1	13	—	12	6	32
Social Psychology.....	2	12	—	9	3	26
The Community.....	1	15	1	4	3	24
Population.....	1	10	—	7	5	23
Introduction to Social Work.....	—	17	—	5	1	23
Social Control.....	—	13	—	8	2	23
Educational Sociology.....	1	11	—	3	7	22
Juvenile Delinquency.....	—	9	—	7	2	18
History of Social Thought.....	—	13	—	1	1	15
Social Aspects of Child Welfare.....	1	9	—	1	1	12
Introduction to Public Welfare.....	—	1	—	7	5	13
Social Studies (Orientation).....	—	4	—	1	5	10
Social Progress (Change).....	—	5	—	2	2	9
Introduction to Social Case Work.....	—	4	—	3	2	9
Contemporary Social Movements.....	—	3	—	5	—	8
Methods in Social Research.....	—	8	—	—	—	8
Ethics.....	3	4	—	—	—	7
Social Ethics.....	—	3	—	2	2	7
Principles of Social Organization.....	—	5	—	1	1	7
Social Adjustment.....	—	5	—	1	1	7
Seminar.....	1	4	—	1	—	6
Social Studies.....	2	3	—	—	1	6
Modern Social Theories.....	—	4	1	—	1	6
Sociology of Conflict.....	—	4	—	—	1	5
Poverty and Dependency.....	—	3	—	—	2	5
Indians of North America.....	—	1	—	4	—	5
Readings in Sociology.....	—	1	—	2	2	5
Teachers Course.....	—	—	—	5	—	5
Sociology of Childhood.....	—	—	—	5	—	5
All Others.....	8	78	1	17	11	115

lems. In the South Atlantic-South region Marriage and the Family is offered more frequently than Social Problems.² Professor

²In an analysis of the catalogues of 60 four-year colleges and universities of the same region of the country Kutak verified this finding. He found 65 courses listed in the 60 schools in General Sociology, 59 in Marriage and the Family, 41 in Social Problems, 39 in Criminology and Delinquency, 36 in Rural Sociology. Kutak, Robert L., "The So-

Groves and his pioneering work in marriage at the University of North Carolina may have influenced this development in the region. Marriage and the Family runs a close second to Social Problems in all regions. Rural Sociology is almost as popular as Marriage and the Family. If Juvenile Delin-

ciological Curriculum in the Southeastern States," *Social Forces*, October, 1945, pp. 56-66.

quency is combined with Criminology [seventy courses are listed,] this area is close to Rural Sociology and Marriage and the Family in importance. Race Relations, Urban Sociology, Social Psychology, The Community, Population, Introduction to Social Work, Social Control and Educational Sociology come next in importance and are offered by approximately one school in six. No other courses are offered by more than fifteen schools. Fifty-four courses are given by one school only. (See Table 2 for titles listed by 5 or more schools.)

Teachers colleges are giving considerable work in the field of social work. Introduction to Social Work is the most frequent and popular course. Child Welfare and Introduction to Public Welfare are next, and some of the schools offer Introduction to Social Case Work. In addition several more specialized social service courses are listed.

Several of the individual teachers colleges have developed very complete and well-rounded departments of sociology. A few have course offerings and staffs comparable to those in our larger universities.

CONCLUSIONS

1. Teachers colleges place greater emphasis upon applied sociology than upon theory or research.

2. With the increased emphasis upon the social studies in high schools, colleges will be encouraged to increase their offerings in sociology, especially in those regions where sociology is not being offered at the present time.

3. Those engaged in the training of graduate students for college teaching should be in a position to offer guidance to these students. If they are primarily interested in teaching we should prepare them to teach the type of sociology courses that will aid them as they go into the high schools. Graduate students who are more interested in teaching than research should consider the opportunities for teaching in the teachers college.

4. Those teaching in teacher training institutions should keep in mind that although

sociology is a coming subject in the secondary curriculum very few high schools offer enough courses so the teacher can teach sociology only. It is a one semester elective course in most high schools. Only the largest high schools offer sufficient sections to make it possible for a teacher to teach sociology alone. College professors who are training high school teachers have a responsibility in guiding students toward a broad general training in all the social sciences so that they will be prepared for the work they will have to do. Too many department heads in history, economics, government and sociology build up the number of majors in their departments when a field major in the social sciences would be of far more value to the student going out to teach the present social studies subjects at the secondary level.

5. If sociologists feel that they should have a part in shaping the social thinking of the next generation there is no greater field of opportunity open than that presented by the teachers colleges. The students in our teachers colleges are the ones who will be directing the social thinking of the high school students of tomorrow.

6. If the public schools are to follow the Springfield Plan and try to do something to lessen racial and nationality prejudice and friction then it would seem that the sociologists should give more thought to the development of courses in intercultural relations for undergraduate and graduate students. These courses should be given much more emphasis in the teachers colleges than they receive at the present time. Only one school in four now offers such a course.

7. Finally, if our discipline is to be accepted we must assume a greater responsibility in training graduate students to do effective teaching. We have impressed students with the importance of research and writing. This is very good. However, many graduates will spend most of their time teaching and we have as great a responsibility in training them to teach in an effective way.

OFFICIAL REPORTS *and* PROCEEDINGS



ANNOUNCEMENT OF ELECTION OF NEW SECRETARY-TREASURER AND MAN- AGING EDITOR

Dr. Ernest R. Mowrer was elected Secretary-Treasurer of the American Sociological Society for the year 1947, and Managing Editor of the *Review* for Dr. Taeuber's unexpired term ending June 30, 1947, and then for the term ending June 30, 1949. Dr. Mowrer's official address is, Department of Sociology, Northwestern University, Evanston, Illinois. All business communications concerning either the Society or the *Review* should be sent to Dr. Mowrer.

ANNOUNCEMENT OF NOMINATING COMMITTEE

President Louis Wirth has designated the following as members of the Nominating Committee for the year 1947:

James A. Quinn, Chairman—University of Cincinnati

George B. Vold, University of Minnesota.

Gordon W. Blackwell, University of North Carolina.

J. H. Kolb, University of Wisconsin.

Clyde W. Hart, 2517 Mozart Place N.W., Washington, D.C.

Elbridge Sibley, 726 Jackson Place N.W., Washington, D.C.

Ray E. Baber, Pomona College, Claremont, California.

Leslie D. Zeleny, Colorado State College, Greeley, Colorado.

Walter T. Watson, Southern Methodist University, Dallas, Texas.

Edgar T. Thompson, 138 Pinecrest Road, Durham, North Carolina.

Samuel M. Strong, Carleton College, Northfield, Minnesota.

Mary Schaffler, Western Reserve University, Cleveland, Ohio.

Ernest Manheim, University of Kansas City, Kansas City, Missouri.

A. J. Jaffe, 4604 Jones Bridge Road, Bethesda 14, Maryland.

Carroll D. Clark, 643 Indiana Street, Lawrence, Kansas.

CURRENT ITEMS



NEWS AND ANNOUNCEMENTS

UNESCO. At the final business meeting of the American Sociological Society on December 30, 1946, the incoming president was instructed to appoint a three man committee to cooperate with the American delegation to UNESCO. Members of the Society who wish information with respect to the activities of UNESCO may address the Division of Public Liaison, State Department, Washington. Those who have proposals or projects they wish to submit may address Mr. Charles A. Thompson, Director of UNESCO Staff, State Department, Washington.

American Jewish Congress. The Commission on Community Interrelations of the American Jewish Congress has announced a program of grants-in-aid to university students for research on minority group problems. The Commission is particularly interested in supporting projects in the following areas: (1) development of objective, quantitative measures of the amount of discrimination against various minority groups, and (2) studies of the effects of contact between members of different groups on intergroup attitudes and behavior. Ten thousand dollars have been appropriated for grants during the academic year 1946-47, with one thousand dollars the maximum grant for any single project. Grants will include provision for research and field expenses but not for general support of the student during the project. Requests for information should be addressed to Dr. John Harding, Commission on Community Interrelations, 212 West 50th Street, New York 19, N.Y.

Carnegie Corporation. President Devereux C. Josephs has announced that the corporation is interested in considering for foundation support educational projects at the college level in the field of international relations.

The Middle East Institute announces the publication of a new quarterly, *The Middle East Journal*. Harvey P. Hall is the Editor and Harold W. Glidden of the Library of Congress is the Advisory Editor. Communications may be addressed to 1906 Florida Avenue N.W., Washington 9, D.C.

The National Conference for Prevention and Control of Juvenile Delinquency was held in Washington in November, 1946 under the auspices of the U. S. Department of Justice. One thousand delegates were present. A Continuing Committee

made up of the Conference Executive Board, 16 panel chairmen and nine young people, aged 16 to 25, was established to carry on the campaign against juvenile delinquency.

National Planning Association. Formation of a Committee of the South, made up of Southern leaders, to work out private and public programs for speeding the development of the South's vast resources has been announced by the Chairman of the Board of Trustees of the National Planning Association, H. Christian Sonne.

Social Security Administration. Roy L. Roberts recently transferred from the U. S. Department of Agriculture to the Social Security Administration where he will work specifically on the problems associated with the extension of the social security program to farm people. Mr. Roberts was with the Bureau of Agricultural Economics, U. S. Department of Agriculture for seven years, the last three as a regional leader for the Division of Farm Population and Rural Welfare.

Associated Colleges of Upper New York. The Department of Sociology announces the following appointments for the current academic year: Evelyn B. Crook, Head of the Department; Roy W. Foley, Professor; Meyer Barash, Assistant Professor; Carl Knudsen, Assistant Professor; Margaret Kilcawley, instructor; Anne Michaelis, instructor.

Bard College. Lyford P. Edwards, for 28 years Professor of Sociology, has been granted sabbatical leave until July first when he becomes Professor Emeritus.

Pennsylvania State College. Seth W. Russell has been promoted to Professor of Sociology and has been appointed Assistant Dean of the School of the Liberal Arts.

Clarence W. Anderson, formerly of Wayne University and former Executive Secretary of the Metropolitan Detroit Fair Employment Practices Committee, has joined the staff as Instructor in Sociology.

Princeton University. Graduate courses in sociology have been inaugurated at Princeton for the first time this year. A more complete program of graduate work will be established by next year, and

opportunities for training and research will be enhanced by cooperation with several research organizations of the University: The Office of Population Research, the Industrial Relations Section, and the Office of Public Opinion Research.

University of Delaware. Dr. Frederick B. Parker, formerly of the Pennsylvania State College, has assumed the chairmanship of the Department of Sociology. Mr. Lincoln Armstrong has joined the staff as instructor in Sociology.

University of Illinois. Professor E. T. Hiller returned to the Department this year after having been on leave of absence for the year 1945-46, during which time he was located at Tucson, Arizona.

Professor D. R. Taft is on leave for the first semester of the current year. During the term Professor Taft is conducting interviews with returned civilian and military occupation officers as a basis for a study of the reactions of the German and Japanese people to certain aspects of American Military Government.

Professor Florian Znaniecki delivered a paper on "Exclusive Group Loyalties as Factors of International Conflicts" at one of the meetings commemorating the 200th anniversary of the founding of Princeton in October.

Dr. Robert Janes, who received his Ph.D. in sociology at Illinois in 1941, has returned to the Department as instructor in Sociology. He spent the last eight months of his military service in Tokyo.

Dr. Richard Dewey, recently of the Milwaukee City Planning Council, and later Assistant Director of the Fox River Valley Regional Survey of the Illinois Postwar Planning Commission, has joined the Department this year as Visiting Assistant Professor of Sociology.

Whitman College. Dr. Egon E. Bergel has joined the sociology and economics department as associate professor. Having received his doctorate at

Harvard University, he was with Friends University and was control editor for the Office of War Information before accepting the Whitman appointment.

Dr. S. Kirson Weinberg, who has joined the sociology department of Whitman College as assistant professor, is completing a study on "The Mental Hospital as a Social Institution" with Dr. H. Warren Dunham of Wayne University. He is also conducting a project on rural institutions and voluntary associations.

SEBALD RUDOLPH STEINMETZ, 1862-1946

Word has been received of the death of Dr. S. R. Steinmetz, the eminent Dutch sociologist. He was best known for his work in the fields of legal ethnology, nationalities, and war. His doctor's dissertation at Leiden (1892) was entitled, *Ethnological Studies on the Early Evolution of Punishment*. In 1903 he published his *Legal Relations of the Native Peoples of Africa and Oceania*. Two books on the nationality problems of Europe were published in 1920 and 1927. His *Philosophy of War* in 1907 was followed by *Sociology of War* in 1931. His bibliography contains six other books on various subjects. He contributed three articles to the well known *Handwörterbuch der Soziologie*, edited by Alfred Vierkandt, and numerous articles to journals.

Dr. Steinmetz was a life long resident of The Netherlands, except for a short period as a student at Leipzig. He taught successively at Utrecht, Leiden and Amsterdam Universities. He retired in 1933. The fact that the chairs he occupied were in the fields of ethnology, sociology, social geography and cultural anthropology, testifies to the breadth of his competence.

BOOK REVIEWS



The Use of Personal Documents in History, Anthropology, and Sociology. By LOUIS GOTTSCHALK, CLYDE KLUCKHOHN, and ROBERT ANGELL. New York: Social Science Research Council, 1945. 243 pp.

The authors whose studies are included in this volume had a common task, *i. e.*, to report and appraise the use that has been made in the disciplines which they represent of a kind of source material which is common to all three: the human or personal document" (Redfield, p. VII). We shall consider, therefore, their work as a joint contribution to the methodology of scientific research in the total empirical domain which these disciplines cover.¹

The primary methodological problem under discussion is the reliability of this common material as a source of scientific knowledge. We are puzzled at first by different denotations of the term "document," "human document," "personal document." These differences, however, become understandable if we remember that all "documents" have three distinct aspects.

First, as Gottschalk points out, every document is a human product. As such, it offers manifest evidence that some agent performed the action of producing it. But in order to use this evidence as a source of information, either about the personality of the author or about the culture in which he participated, the scientist must ascertain when, where, under what conditions it was produced. Historians often find this a difficult task and give it considerable attention (Gottschalk, Ch. III). Scientists who obtain documents from their contemporaries avoid this difficulty, but face another problem. When an author gives a written or oral statement under the influence of a scientist, this is a case of social interaction. The author's reaction is conditioned by the action of the scientist; the latter thus becomes a part of the documentary evidence. Sociologists have been aware

of this for some time; but this relationship between the scientist and the author has never been so thoroughly discussed as in Kluckhohn's study.

Secondly, most documents which scientists use contain descriptions of facts. Even if the author observed the facts he describes, the reliability of his descriptions is always questionable. Since in reconstructing the past, historians must use such descriptions, they have developed effective methods of testing the reliability of factual information found in documents. Gottschalk gives an excellent critical survey of these methods. Anthropologists and sociologists have been much less methodical, although the most important factual information which they are using—descriptions by the individual of his own past actions—are especially liable to subjective bias. Angell finds that some progress has been achieved by sociologists during the last 25 years; it is obvious, however, that sociology still lags in this respect behind history.

Thirdly, many documents contain evaluative judgments. Such judgments do not represent any knowledge about facts to be tested; they are facts to be investigated in connection with other facts. Anthropologists and sociologists assume for purposes of research that these judgments are manifestations of socially conditioned attitudes and indicate the author's awareness of some standards of valuation and norms of activity which he and others are supposed to accept as binding. This heuristic hypothesis has led to many important discoveries of cultural patterns and social structures. But historians do not use this material so thoroughly or consistently.² Though Gottschalk mentions it as

¹ Perhaps this deficiency is a result of the inability or unwillingness of most historians to refrain from judging the attitudes and actions of other people by their own standards and norms. Some of them rationalize this unwillingness by the logically defective assertion that non-evaluative study of diverse systems of values implies a relativistic philosophy of values. Most American sociologists and anthropologists no longer confuse scientific objectivity with philosophic relativism.

² The reviewer regrets that he cannot discuss here, in connection with this contribution, Gordon W. Allport's work "The use of personal documents in psychological science" (*S.S.R.C.*, 49, 1942).

valuable to the "historian of opinion" (24-26), he does not discuss fully the problem of its methodical usage.

Even more significant than the criticism of the reliability of documentary material is the methodological discussion of scientific conclusions. Here we notice a remarkable agreement. Increasingly methodical use of "human or personal documents" in recent years seems to lead away from the antithesis of "idiographic" vs. "nomothetic" science. Gottschalk emphasizes that history is becoming both a study of man as participant in collective life and of man as individual creator (72). This implies (if we interpret it rightly) that the application of general laws to collectivities is limited by human creativeness, while the application of the principle of uniqueness to creative individuals is limited by their common participation in collective processes.

Gluckhohn is more explicit. Comparative analysis of personalities as participants in a culture discloses within a given society specific variations by roles and idiosyncratic variations, which were obscured by the assumption of a general and lasting conformity of individuals with the cultural patterns abstractly constructed by anthropologists; on the other hand, it opens the way to broader comparative generalizations, which were impeded by the "obsession" of anthropologists with differences between cultures.

Angell's approach to this methodological problem is most instructive. The common and essential characteristic of all science is the use of empirical data to formulate and test hypotheses. Inasmuch as a hypothesis is a tentative expression of a law, all science is nomothetic; but there is "no reason why the term should not cover laws which have been worked out from and are applicable to individual cases only" (125). It is not the degree of abstractness and the range of applicability which makes a hypothesis scientific, but its exactness and verifiability. The author finds some recent progress both in synthetic studies of individual life-histories (Ch. II) and in analytic studies intended to reach sociological generalizations about processes in which individuals participate (Ch. III). He says, however: "We have great difficulty in framing hypotheses which can be rigorously tested because of the vague character of the concepts in terms of which our hypotheses are formulated" (230). It seems, indeed, that social science will have to develop (as biological science has done) a systematic framework of

well-defined concepts with varying degrees of generality and abstractness to deal with the vast diversity and complexity of empirical data in their domain.

FLORIAN ZNANIECKI

University of Illinois

Fundamental Patterns of Maladjustment—The Dynamics of Their Origin. By L. E. HEWITT and R. L. JENKINS, M.D.; An official document of the State of Illinois, 1946; pp. 110, including two appendices.

This is a study of 500 children admitted (over several years) to the Child Guidance Institute at Ann Arbor, Michigan. The authors (particularly Lieutenant Hewitt) have attempted to delimit, by statistical methods, dominant types of behavioral problems and to establish their relationship to different types of social problems. Of necessity such a project was doomed to failure. The children were from all parts of the State; the data were recorded by a wide scatter of social workers; subjective judgments as to traits and definitions of behavior certainly differed from expert to expert. Moreover every psychiatrist knows that similar social situations have an idiomatic impact upon different individuals.

Recognizing these odds the authors went ahead to list certain items of behavior (of the order of "initiatory fighting," "truancy from home," "sensitiveness," "cruelty," etc.) and to discover which of these had high correlations with others. The effort was honest and guarded with every precaution. The result is clear and convincing—a picture of three syndromes, unsocialized aggressive patterns, socialized delinquency patterns and over-inhibited behavior patterns. Moreover, these show a high correlation with certain social situations—in brief, the first with what would be termed rejection, the second with careless and sloppy parental indifference, and the third with parental over-dominance. The result is all the more amazing because the family was the only part of the background that was taken into consideration. What will this study do for those who claim that the individual's entire life is quite determined by his early, dramatic family experiences!

Each author then contributes an attempted interpretation of "how" and "why." Here the reader is enough at home almost to forget the cold chills stirred by the earlier chapters. And one is further reassured by the chapter on "outcomes" which quite sensibly tells that the un-

socialized aggressive child shows the least tendency to change, the over-inhibited, the best chance for change for the better, the socialized delinquent, a considerable tendency to change but either for better or worse.

It is hard to believe that these findings are final. There is no effort, for instance, to make a distinction between what this reviewer terms the rejected and the unwanted child (for which David Levy uses the terms "primary affect hunger" and "rejection"). Nevertheless the authors—carefully using a statistical approach—have clearly demonstrated that "a large proportion of the children who have been exposed to one pattern rather than another show marked similarities in the type of maladjustment which they exhibit."

One approaches a book just bristling with tetrachoric correlations with many misgivings. Done with it and finding that he had suffered no violence, he just has to wonder whether the favorable impression comes from the new friend himself or from the fact that, for now, the new friend agrees with him. A careful, conservative piece of work of this sort which builds a bridge between two different approaches to the problem is of inestimable value. If the psychiatrist still is utterly amazed that such imponderables can be subject to statistical analysis and if the statistician still is not ready to go beyond a small corner of the whole problem—at any rate what has been done promises a mutually helpful relationship between the two approaches.

JAMES S. PLANT, M.D.

*Essex County Juvenile Clinic,
Newark, New Jersey*

Industrial Relations and the Social Order. By WILBERT E. MOORE. New York: The Macmillan Company, 1946. Pp. 555.

This work is the crystallization from a course in "Industrial Sociology" given by the author at Pennsylvania State College. The book is unique in that it tries to analyze the structure of industry within the whole framework of society with which it is constantly interacting.

The author after first pointing out that industry is a complex social organization which cannot be studied apart from society itself, traces the rise of modern industry. He discusses the origin of the factory system, and the effect of the rise of capitalism, science, technology and the division of labor.

He then goes on to a rather theoretical analysis of management giving the "Blueprint" of or-

ganization, the managerial functions of executives, specialists and supervisors including a brief treatment of "scientific management" and "personnel work."

In "Part 4. Industrial Organization: Labor," the author discusses the conditions for the maximum utilization of manpower. He has included a table showing the major types of waste of labor. In it he has failed to mention the wastes resulting from excessive profits, low purchasing power and some of the other concomitants of the capitalistic system.

He has a good treatment of the worker and the machine as well as a discussion of the employee's relation to status and motivation. Another section deals with unions, collective bargaining and industrial conflict.

Finally, in conclusion, the author discusses industry and society. It is in this section that he makes his greatest contribution. He calls attention to the various kinds of industrial communities and the fact that one cannot adequately study industry without taking into account the community in which it is located. He discusses social classes, social controls, and the prospects and problems of economic planning.

My major criticism of this book is that it is too theoretical and does not illustrate its points with practical examples. For instance, it treats of unions and strikes but almost without mention of concrete situations. This fault goes all the way through the book. It thus becomes a treatise which students can use as collateral reading but not one that they could advantageously use as a text-book.

My second criticism is that the analysis is far too brief in its treatment of many important issues. For instance, the author describes why labor unions have so often found themselves opposing the forces of law and order in these words, "the difficulties being partly doctrinal and partly political in the narrow sense of effective power." This, of course, is far too inadequate an explanation for the student. Again, the author's championship of compulsory arbitration is of doubtful validity and is so brief it is of little value. One of the best chapters is that on the "social controls of industry," although even here the treatment is extremely brief.

The book is refreshing because it does puncture many shibboleths and myths of the dominant capitalistic system. Thus the author writes of the belief of the capitalists that America has a society in which the individual has complete freedom to rise because of his ability. He shows

that this universally accepted capitalistic belief is largely a myth. Again he demonstrates that the popular slogan, "Keep government out of business" is largely a shibboleth to enable corporation executives "to control other people's money as they see fit."

On the whole, the author deserves great credit for pioneering in a sociological approach to the field of industrial relations. It is to be hoped that at some future time he may sharpen his chapters by concrete examples.

JEROME DAVIS

West Haven, Connecticut

The Human Problems of an Industrial Civilization. 2nd ed. By ELTON MAYO. Boston: Harvard University Graduate School of Business Administration, 1946. 194 pp. \$2.00.

The Social Problems of an Industrial Civilization. By ELTON MAYO. Boston: Harvard University Graduate School of Business Administration, 1945. xvi + 150 pp. \$2.50.

Industry and Society. Edited by WILLIAM F. WHYTE. New York and London: McGraw-Hill Book Co., 1946. 211 pp. \$2.50.

Some fourteen years ago Professor Elton Mayo published a book, *Human Problems of an Industrial Civilization*, that was something of a pioneer work in a field now called, at least by sociologists, "industrial sociology." The publication last year of a second work, *Social Problems of an Industrial Civilization*, was made the occasion of a new printing (not a new "edition") of the earlier work. As a result of these works, and of other studies summarized in them, the author has been hailed by *Fortune* magazine as a sort of pragmatic philosopher of American industrial relations.

The essential problems that give unity to the two books are familiar to every sociologist or student of an elementary course in sociology. They concern the role of the individual in a multi-group society; the increasing importance of secondary relationships; the problems of institutional balance in an urban, industrial acquisitive society. The one-word change in title is partly symbolic of differing emphasis—in the one case, what might be called social-psychological, in the other, sociological.

Of Mayo's two books, the second must be assumed to be the less familiar to the scholar, and in any event the more interestingly controversial as viewed by the sociologist. In the *Human Problems* . . . the author documented the significance of the individual's "definition of

the situation" (which is always to be understood as "evaluation of the situation" also) with respect to industrial work—fatigue, monotony, working conditions, the supervisory situation, and relationships with fellow employees. The book concluded with three chapters attempting to assess the significance of industry for the character of society as a whole. It is to this latter theme that *Social Problems* . . . returns. In the interim, however, the author's voice has become shrill.

It is proposed to approach *Social Problems* . . . as a tendentious tract rather than as a contribution to learning. This is in keeping with the author's own procedure. Of the book's six chapters, only three represent reports on social research undertaken in industry. Chapter III summarizes research reported more fully in an article and in Chapter II of *Human Problems*. . . Chapter IV again summarizes the Hawthorne Western Electric study, which has already accounted for four entire volumes and a major portion of two others besides summary papers too numerous and too repetitive to mention. Chapter V reviews a study of absenteeism published recently as a monograph. These chapters are sandwiched between introductory and concluding chapters that present the author's views on management, society, sociology, politics, the causes of war, and the nature of scientific method.

Professor Mayo's views as to the proper course of social scientific inquiry may be most succinctly summarized as radical empiricism. This position rests on two fundamental misunderstandings: (1) The author is ignorant of the role of theory in social research. Rather, he advocates amassing observations, apparently at random. At no place does he indicate how it is that one knows where to begin observing the infinite phenomena of the universe. (2) He is ignorant of the difference between science and technology, and thus is insensitive to problems of ends or values. He pleads for knowledge of the techniques of cooperation, which will settle all issues from those in industry to those in international affairs. Cooperation toward what goals, with what inducements, under whose direction, with what safeguards for participants? The direction and use of research in industry is not so simple as in the case of medicine, to which the author frequently and mistakenly refers as a science rather than as a useful art.

Professor Mayo is at pains to condemn sociology, apparently without any marked effort to

inform himself on the subject. The overstatement, the generalization of the extreme instance, is the author's stock in trade. He writes, "Sociology is highly developed, but mainly as an exercise in scholarship. Students are taught to write books about each other's books. Of the psychology of normal adaptation, little is said, and of sociology in the living instance, sociology of the intimate, nothing at all." (P. 20.) A little observation, in any issue of this journal for example, is the physician's own medicine and seems indicated. Pontifical utterances are not confined to statements about the social sciences. Thus, "And no university calls attention to the fact that material provision is only one of the duties of civilization, the other being the maintenance of cooperative living." If a corporation can be libelled, Professor Mayo should be sued by his employer.

As noted, the empirical studies reported in *Social Problems* . . . are available elsewhere. The sound conclusions have already been drawn. On balance, the second book detracts from the earned reputation of the first by pointing up some of the shortcomings of the whole approach.

A more positive contribution to the growing literature on industrial sociology is provided by a little symposium, *Industry and Society*, edited by Professor William F. Whyte. The eight substantive chapters range from an extremely elementary discussion of "The Factory as a Social System" by Burleigh B. Gardner to a systematic and sociologically informed discussion of the "Functions and Pathology of Status Systems in Formal Organizations" by Chester I. Barnard. Aside from very brief introductory and concluding chapters that are unsigned, the remaining papers fall into two groups: (1) those that summarize intensive investigations—"The Factory in the Community" (Newburyport, Mass.) by W. Lloyd Warner and J. O. Low, and "When Workers and Customers Meet" (restaurant business) by William F. Whyte; (2) those that represent analyses of particular phases of the industrial situation—"The Motivation of the Underprivileged Worker" by Allison Davis; "Race Relations in Industry" by Everett C. Hughes; "Role of Union Organization" by Mark Starr; and "The Basis of Industrial Conflict" by Frederick H. Harbison. The book as a whole is less consistent in point of view and method than the first chapter claims and considerably less comprehensive than the title promises, but is useful for professional and practitioner alike.

WILBERT E. MOORE

Princeton University

The Nuremberg Trial and Aggressive War. By SHELDON GLUECK. New York, 1946. 121 pp.

Twelve of the twenty-four defendants are now dead. Most of the rest, we anticipate, will live out their lives in prison. But discussions of the legal and moral bases of the trials will still go on, for our consciences are still troubled.

This book attempts to provide a reasoned justification of the Nuremberg trials and to show that it was justice that they administered. Many aspects of the trials are necessarily considered, but the argument is mainly centered on their really crucial issue, the accusation that a crime was committed in "the planning, preparation, initiation, and waging" of aggressive war.

That the questions involved are difficult is shown not only by the abundant literature of protest already produced, but by the change of mind which the author himself has undergone on the crucial question discussed. But in another sense the issues are narrow, if considered in terms of the alternative courses of action that were in fact available.

On one point there has been at all times agreement. We could not leave the leaders of our defeated enemies at large. Their offenses included a deliberate and systematic ferocity and a conscious rejection of moral restraints, combined with a skill in manipulating opinion, that were new in the history of mankind. We were justified in providing safeguards against their reconquest of power. If we were then to take action against them, the alternatives to the course adopted were only two. We could execute or imprison them without trial, by "political" or "executive" action. Or if trial was to be used, we could limit the trial to offenses and to a procedure for which precedents could be found.

The decision to use a trial seems clearly a wise one, especially now as the tedium it produced wears away. If our appeal is to the judgment of mankind and in a sense to later history, why not hear the defendants? The record is now overwhelming; the defendants could not shake it; it will stand as long as human records are preserved. If Germans are not impressed by the spectacle of Allied justice and if the only purpose of the defendants was to abuse it, we have ourselves to impress and our own standards of fairness to enforce. The alternative of shooting and imprisonment on a mass scale, by "executive" action, seems wholly unacceptable.

The insistent question remains whether four nations, through action of their executive branches, can create an international criminal court and at the same time define the interna-

tional "crimes" within its competence. This question would remain, and could not be avoided, even if the second alternative were adopted and criminal liability restricted more narrowly. The reasoning of some critics of the trials would wholly exclude the possibility of convoking such a tribunal and would remit the defendants, and all other charged with similar misconduct, to trials under existing national law by courts in the localities where the acts themselves were committed. Since much of the conduct involved, particularly the murder, looting, and imprisonment of civilians, was authorized by the law then in force, the problem of retroactive legislation would necessarily arise, unless the legal standards of Nazi Germany itself were to govern in the trial of men whose offense was the creation of those standards. Legal technicality carried to this extreme, accepting so completely the exclusive power of the national state to declare and create law, was rightly rejected. Though its legal source is essentially the will of the victorious powers, the Nuremberg court is a court. Though the crimes (violations of the laws of war and crimes against "humanity") defined in counts three and four were given immunity or were expressly authorized by Nazi law at the time, they are condemned so universally by the legal systems of modern times that their punishment seems proper and just, however novel the means.

We are left with Count Two and its "crime" of aggressive war. To show that this "crime" was known at the time the acts were committed, the author reviews the international declarations preceding 1939, particularly the "renunciation" of war in the Kellogg Pact of 1928. He argues at length that the "crime" can be committed not only by states, the normal subjects of international obligations, but by individuals through whom states must act; the immunity of individuals for acts of state he explains as essentially a rule of convenience applicable to normal and peaceful intercourse, reflecting at most the inadequacy of international agencies for legislation. He demonstrates convincingly the applicability of international law to individuals, in the areas like piracy to which international prohibitions have been accepted through usage. The issue of retroactivity he explains, quite correctly, as a broad issue of fairness, in which notice to the accused plays an essential part. The prohibition of *ex post facto* legislation incorporated in the constitutions of western states is an attempt to give precision to a much older idea, whose application can in no case be purely me-

chanical. The issue of retroactivity, in any event, merely presents in reverse form the main issue, whether the essential elements of criminality had been fully and fairly spelled out, through a published prohibition with which individuals could be expected to comply.

On the main issue it seems that the case is not proved. The language of the Kellogg Pact and similar declarations is at most extremely ambiguous. It seems clear from both language and context that no responsible statesmen and none of the populations concerned intended before 1939 the liabilities now being imposed. Though one may accept the continued power of usage to generate law and reject the state's monopoly of legislation which modern political and legal theory has so largely conceded, it is surely not enough to show that during the present century "a widespread custom has developed among civilized States to enter into agreements expressive of their solemn conviction" that unjustified war is criminal.

But the ultimate judgment on the Nuremberg trials must depend on a still broader issue of morality, to be resolved in the course of an uncertain future. Mr. Justice Jackson, who contributes a foreword to the present volume, has himself shifted between claims of present legality and claims that the principles enforced in the trial are a new contribution to international regulation. The vital question is whether we, as principal authors of this contribution, seriously intend it with all its implications. This would mean the assumption of similar liabilities by ourselves and other members of the United Nations for similar acts in waging aggressive war. It might mean, ironically enough, the extension of similar liabilities to Russia for its unprovoked attack on Japan in August of 1945 as well as for its earlier attacks on the Baltic countries. It would certainly mean the development of international controls to a point far beyond their present stage, to such a point that the conduct for which we hang Nazi and Japanese leaders is truly controlled and effectively condemned. If the justice becomes equal justice, our consciences may yet be satisfied.

The argument of Professor Glueck is spirited and its range is wide. His book is the ablest case for the prosecution yet presented. It presents the trials in their proper perspective, and shows them to be, as they are in fact, the most crucial test we have faced of the foundations of our legal and moral ideas.

JOHN DAWSON

University of Michigan Law School

Soviet Philosophy: A Study of Theory and Practice. By JOHN SOMERVILLE. Pp. XI, 269. Philosophical Library, New York, 1946.

In the flood of books on Soviet Russia now appearing this one occupies a distinctive place. So far as I know it is the most comprehensive, concise and lucid exposition of the whole of Soviet philosophy available in English. It is fairly objective, although there is little criticism of Soviet practice, and the soft pedal is put on its cruelties and inconsistencies. It deals, in the order named, with the following topics: I. The Theory of Historical Materialism; II. Political Life; III. Soviet Ethics: Socialist Humanism; IV. The Arts; V. Our Universe: General Theory of Dialectical Materialism; VI. The Human Mind: The Dialectical Method of Thinking; VII. Pivotal Controversies in the History of Soviet Philosophy; VIII. Dissemination of Philosophy in the U.S.S.R.; IX. About Reading.

I consider the exposition of the Soviet World View to be the best that I have seen. Dr. Somerville makes it very clear that this is a strong, well-knit and all-inclusive system, which omits no principal phase of philosophy. After reading his book no intelligent person could be in any doubt as to what Dialectical Materialism means; as a method of thinking, an ethico-social philosophy, and an ontology. In the last respect, since it rejects mechanistic materialism, it is really a doctrine of Emergent Evolution. It is a metaphysics of Universal Becoming, with an upward-pushing impetus. In human society this impetus becomes transformed into the Class Struggle, which will inevitably end, through the victorious Dictatorship of the Proletariat, in a classless humanistic society. The basic assumption, of course, is that the main driving force of social change is the economic struggle, although other values supervene thereupon.

In his treatment of Formal Logic vs. Dialectical Thinking Dr. Somerville is distinctly unfair and one-sided. The principles of formal logic, the "Laws of Thought," are not ontological principles and no good logician claims that they are such. *They are simply formalized statements of the conditions of rational discourse or consistent thinking.* Throw overboard the principles of Identity, Non-contradiction and Sufficient Reason, then thinking ceases to be coherent and becomes a veritable witches cauldron in which fair is foul and foul is fair and anything can be asserted to be its opposite. The dialectical method, borrowed from Hegel, with the Economic Drive substituted for the World Spirit,

is not a logic in the proper sense of the term. Hegel's sophistically called "Logic" is, not a logic, but a Metaphysics of Becoming. To throw away the principles of straight thinking is to open the door to all sorts of evasions, dodges and the bland assertion that Freedom consists solely in the conformity of the individual, in his thinking, speech and action, with the social order which is ruled by the class-conscious minority of the proletariat; i.e., by the commissars, the Bolshevik Party, and, in the last analysis, by the Politburo and the N.K.V.D. Hegel had a similar Pickwickian conception of freedom and morality. He said: "to be moral is to act in accord with the moral tradition of one's country." But Hegel did not go so far as the Bolsheviks. He held that art, religion, science and philosophy are *above the State*, whereas for the Soviets they are organs of the state. All the "freedoms" in the U.S.S.R. function only within the limits laid down by the vigilant watchmen installed by an all-powerful or totalitarian state, which, according to the philosophy, is preparing the way for the Heaven-on-Earth of the stateless, classless society. In spite of Dr. Somerville's contentions I maintain that the U.S.S.R. is totalitarian. I know what I mean by freedom; I mean the right to say, if I think so, that the President of this republic is a fat-headed old fool and that he is leading the country to the "demnition bow-wows." What would happen to a Russian who said that of Stalin or the Politburo?

Let us not fool ourselves. We are between the devil and the deep sea. Collectivism is making head in the U.S.A. because, more and more, we are becoming mere puppets and tools and victims of two antagonistic Gigantisms—the gigantism of Big and Monopolistic Business and the gigantism of monopolistic Labor Unionism. I do not know the answer, but I am deathly afraid of the monolithic State. Let us hold on to what freedoms we have, and not surrender to a philosophy in action that knows all the answers and tells us just where to head in, or else—! My basic objection to Sovietism is that it is a dragooned system of mass-control, in which the dissenting individual gets short shrift. The State being a necessary evil, I refuse to trust even the unending audacity of elected persons. Every competent social philosopher or sociologist will admit that personality is socialized individuality, that even our idiosyncrasies are socially conditioned. But this dilemma remains for us all—either the individual is to become a robot; or

there is to remain an area of moral and spiritual self-determination of the variant persons; and we must continue to say to the State: "thus far shalt thou go and no farther!"

In one basic respect the Soviet philosophy is superior to all conservative systems—in its recognition of the dynamic and changeable character of social reality. On the other hand, it is erroneous in two respects. It dogmatically and onesidedly affirms economic processes to be the universal moving spring of social process, and it asserts that this spring operates solely by a Dialectic; that is, by the ceaseless emergence of opposites or contradictions and the overcoming of these—the Class Struggle. Hegel never explained what was to follow the final synthesis of Universality and Individuality in the Germanic *Weltanschauung*. No more do the Marxians explain how the one-party Communist State will develop into the stateless, classless millennium, in which any peasant woman will, off-hand, give a ready answer to any problem of government or production and distribution. And that is precisely *not* the way Sovietism is going. Dialectical Materialism is just as onesided as Hegelian Dialectical Spiritualism. There is dialectic in historical process, but the latter is too complex to be threaded on any one formula. There is a complex plurality of social forces operative in history—human responses to the physical environment, the cake of custom, myth, magic and religion, tribalism, familism, the lust for power, as well as hunger, love and patriotism; not to forget art, adventure, workmanship; in short the undying urge of individuality. Against the monolithic State in all its forms, I opt for "the Universality of the Unique" (with H. G. Wells).

JOSEPH A. LEIGHTON

The Ohio State University

The Population of the Soviet Union: History and Prospects. By FRANK LORIMER. League of Nations, Geneva, 1946. Pp. XIV + 289.

Dr. Lorimer has done the English-speaking world a genuine service in bringing out this study on the population of the Soviet Union. It is a real contribution to the understanding of the position of the Union in the modern world. It gives a clear and comprehensive picture of the factors which have controlled Russia's population growth during the past 2½ centuries and makes use of this knowledge, as well as other developments in modern demography, to indicate the probable course of growth during the

next generation, which is essential if we are to anticipate the form of things to come.

However, this look ahead could not be convincing without a discussion of the geographic and cultural factors which have played important roles in the development of Russia's population, and which will continue to be important although some of these factors are changing rapidly. The effects of the changing economic structure of the nation on population growth is also discussed. Naturally, these basic but preliminary matters had to be treated very briefly with emphasis upon those aspects which affect the rate of population growth. The picture of the Soviet Union's population drawn here seems to the reviewer both more clear and convincing because of this brief excursion into history which at first glance might not be thought germane to a study of current demographic problems.

The body of the monograph is, of course, taken up with the growth of population since the outbreak of World War I and with the description of the changes in the composition of the population since that time. The reviewer knows of no account of the effects of war, famine, and disease on population growth which shows more clearly the deadly roles they play even today than that given in Chapter III. But lest the reader conclude that such devastation of a population by these ever-present enemies of man means that Russia's population will not grow rapidly in the future, he should follow through the discussion of mortality, natality, and future probabilities of growth in Chapters VII, IX, and XI. The data presented here cannot fail to convince him that the population of Russia is likely to become a rapidly increasing portion of the population of Europe in the next few decades. It is true that at times the author is driven to make large use of estimates, but the reviewer believes he has been *reasonable* in his assumptions and calculations so that his conclusions are valuable as a basis for thinking about the Union's future in Europe and in the world. The reader who has given only casual attention to world demography will almost certainly be amazed at the prospective growth of the Soviet Union in relation to that of the remainder of Europe and of North America. He will also be prepared to think more realistically about the Union's political position in the world during the remainder of this century.

Of almost equal importance with the increasing size of the Union's population are the

changes in the composition of this population which are now taking place—the increase in the proportion of the urban population, the growth of workers in manufacturing, transportation, and the professions, and the redistribution of population taking place as the Union organizes to make use of the varied resources of the nation scattered over it so unevenly (chapters IV, VI, VIII, and X). These changes in composition are as clear evidence of a changing economic structure as the figures on production and even more convincing than the elaborate Five Year Plans launched with so much fanfare.

In Chapter V the ethnic composition of the Soviet people claims attention. One cannot read this without marvelling at the unity of purpose and action the Soviet Union showed in the face of national disaster. Perhaps it is fortunate that the Germans did not penetrate far into areas containing large numbers of the more "backward" ethnic groups although the Union's treatment of minor ethnic groups since the Soviets came in power must be given a great deal of the credit for the steadfastness of these minorities in the face of invasion.

I need scarcely say in conclusion that I regard this as a book of first importance, both because of the importance of the subject, and because of the excellence of the treatment. It maintains the high standard of scholarship we have come to expect of Dr. Lorimer.

The League of Nations under whose general sponsorship it was undertaken, and the Office of Population Research at Princeton which was in immediate charge, are to be commended for having made this study possible.

WARREN S. THOMPSON

Scripps Foundation, Miami University, Oxford, Ohio

European Population Transfers, 1939-1945. By JOSEPH B. SCHECHTMAN. New York: Oxford University Press, 1946. Pp. 532. \$5.00.

This is the first intensive study dealing with war-time redistribution of European population to follow the early preliminary report by Eugene L. Kulischer (*The Displacement of Population in Europe*. International Labor Office: Montreal, 1943). Mr. Schechtman's work, unlike that of Kulischer which surveyed all types of movements, is confined to population transfers by which is meant organized removals of ethnic groups from their countries of residence and their subsequent resettlement in territories under the sovereignties of their respective ethnic

homelands. The political situation in Europe before and during the recent conflict, marked as it was by disillusionment and cynicism toward the doctrine of self-determination of peoples, fostered population transfers on an unprecedented scale. The author has embraced all of these in his study, including those effected by Germany, Russia, Finland, Sweden, and the Balkan nations. German repatriation movements receive the major share of attention, in part because of the greater abundance of information available on the program of the Reich. The logic of this emphasis rests also on the fact that German policy set the pattern for and precipitated most of the transfers carried out by neighboring states. The treatment of the repatriation efforts by other nations is as full as the author's data permit.

The materials upon which this study is based consist of news reports, speeches, and published documents. Since sources of this kind are irregular both as to reliability and completeness, the findings are limited at many points. How much more adequate the account might have been had the author delayed publication long enough to secure whatever information is turned up in the military occupation of the defeated nations remains to be discovered. In any case, Mr. Schechtman through a very astute handling of the data has pieced together a cogent description and analysis of the transfers that occurred.

Each occasion of transfer is first examined against the background of its peculiar social and political context in the country of residence. The process of movement is then followed carefully from the administrative organization to handle the transfer, through the transportation, feeding, housing, medical, and financial problems involved, to its conclusion in resettlement. A very full and critical examination of the German resettlement program is presented. Where possible the author endeavors to discover the effects on local economies of the withdrawal of large numbers of repatriates, as in the cases of Estonia and Latvia. But the rapidly shifting political situation obscured from view most such results. The attempt is also made to determine the success of transfers from the standpoint of the individuals concerned as well as from that of the states conducting the renewals. The author's ability to penetrate the fog of propaganda in this connection is noteworthy.

While the mechanics of population transfer is in itself a matter of intense interest, the repatriation movements have the added significance of

political experiments in the solution of minority problems. Unfortunately, the chaos attending the defeat of Germany and her satellites prevented many of the experiments reaching maturity. In the last chapter, however, Mr. Schechtman summarizes what in his view are the lessons to be gleaned from the European experience.

The concluding chapter, in fact, reviews the entire European minority problem as a basis for suggesting an appropriate repatriation program. Very likely the reader will be dismayed at the casual disposal of alternative means of resolving such problems. In that event it may be well to remember that the subject-matter of the book is population transfers and not minority problems. Thus the assumption of political expediency of repatriation seems entirely warranted for the purpose at hand.

AMOS H. HAWLEY

University of Michigan

Economic Stability in the Post-War World—The Conditions of Prosperity after the Transition from War to Peace. Report of the Delegation on Economic Depressions: Part II, League of Nations, Geneva, 1945; 319 pp. \$2.50.

This book is the second, and final, part of a report made by the Delegation on Economic Depressions appointed by the League of Nations in October 1937. It was only natural that the Economic and Financial Organization of the League, confronted with the world-wide nature and special intensity and duration of the economic contraction of the Thirties, should devote major attention to investigations of measures "for preventing or mitigating economic depressions." However, it is an interesting commentary on the strength and shortcomings of this undertaking that the work was continued and completed during a war which gave final proof of the impotence and failure of the League.

Very properly the League Delegation shifted the emphasis of its investigation by dealing extensively with the maladjustments produced by the war and problems of post-war adjustment. Actually, the first part of the report of the special Delegation of Economic Depressions, which appeared as a separate volume in 1943, dealt with ways of delineating post-war problems of adjustment. It considered the questions that would arise during the period of transition, with full recognition that the methods used to solve the transitional problems would in large

measure determine the effectiveness of policies to attain economic stability in the longer run. More specifically, suggestions were made in the first volume on forms of international organization to deal with transitional questions which anticipated the setting up of the International Monetary Fund and the International Bank.

The second volume, under review here, gets down to the post-transition period. Though influenced by the facts of the war and the transition, the bulk of the volume could just as well have been written in the late Thirties. This part of the report presents both an analysis of the nature of depressions and policies that might be adopted to combat depressions. Rather than dealing with alternative theories concerning the nature and causes of depressions, the attempt is made to present a fairly definitive evaluation of the problem. The analysis in both volumes, and especially the second, reflects the predominant influence of Keynesian thinking. There are also indications that would lead one to suspect persistent editorial efforts to tone down the Keynesian influence, at least from the standpoint of terminology.

The first section of the volume under review presents an orderly description of the various types of depression which plague our economy, the mechanics of cyclical fluctuations, the international aspects of business cycles, and the structural and strategic aspects of the economy that influence the cycle. It is, in short, a convenient and useful summary.

The second section is a somewhat more extensive treatment of the measures that might be adopted to eliminate or at least to mitigate the cycle. It is assumed that individual governments and international bodies have the responsibility for achieving economic stability and a high degree of resource utilization. The subject is comprehensively and competently treated but there is nothing new in its analysis. By the very breadth of its coverage, the report lends emphasis to the point that there is no short-cut or single key to the solution of the business-cycle problem, but that the answer lies in consistent and energetic policies in every major field of the economy.

Two shortcomings of this report deserve mention. The first lies in insufficient emphasis on the path to more extensive and progressive utilization of economic resources. Although some attention is devoted to this problem major attention is given to countercyclical activities.

The second lies in the fact that this inter-

national body concerned with the international economy should completely ignore that part of the world which is committed to a planned economy. With approximately one-third of the world either—as in the case of the Soviet Union—directly engaged in centralized planning of state-owned productive facilities or having close ties to such an economy, it is highly unrealistic, although probably politic, for the League to ignore this factor.

As to that part of the world with which the League Delegation Report deals, the basic tasks to which the report is addressed are now in the crucible of international consideration. American readers studying this problem will be conscious of the heavy responsibilities confronting this country in attaining domestic and international economic stability. The United States has given recognition to the goals set forth by the League Delegation in the Employment Act of 1946. It remains to be seen how the Council of Economic Advisers, appointed pursuant to the Employment Act of 1946, will propose to reach and maintain the full employment goal and whether the Congress and the people of the United States are prepared to assume this collective domestic and international responsibility.

PHILIP M. HAUSER

Bureau of the Census
Department of Commerce

Plan for Reconstruction: A Plan for Victory in War and Peace. By W. H. HUTT. New York: Oxford University Press, 1944. Pp. 328. \$4.50.

Professor Hutt sees economic restrictionism as a crushing social ill. To throw off its shackles and release the full productive force of competitive capitalism he proposes a plan whereby Britain can make the transition from full production in war to full production in peace. Likely the plan will not be seriously considered. Nor is it likely that either sociologists and economists will agree as to its merits. Nevertheless the detailed project is worthy of serious study, if for no other reason than it demonstrates the careful planning that would be required to reinstate the virtues of *laissez-faire*

and competition in our economy.

The author begins by presenting drafts for three complete bills ready for introduction in Parliament: the Labor Security, the Capital Security, and the Resources Utilization Protection Acts. In the transition to full employment with full use of resources and all productive equipment, the Labor Security Bill is designed to protect the "established expectations" of all workers from a vast pool to which they will all contribute a percentage of their earnings. Similar arrangements for the security of investments will be based upon "established expectations" and paid through a similar system of grants from a pool. His final proposal is a bill "to protect the utilization of resources, natural and produced, from coercive collusive and other restraints . . . in order to promote the fullest use of resources in response to the preferences of the community as consumers." The scheme is to be administered by the Resources Utilization Commission, a permanent quasi-judicial body consisting of judges, economists and men of affairs.

It is difficult to explain to an American audience the midway position of this proposed plan between state socialism and the system of cartels and monopoly already assumed in Britain's economy. The plan strikes the reader as a combination of our Sherman Anti-Trust Law, the Federal Trade Commission and the new Full Employment Bill. Actually it proposes a kind of NRA in reverse and uses the insuring power of the state to secure the consent of all parties to the release of the full productive capacity of a modern industrial economy.

Since real wages and income will rise all along the line it is felt that no one will lose and the consent of all will be secured for a proposal that uses the powers of the state to secure the assumed virtues of *laissez-faire* and obviate the need for state capitalism. Or possibly this is state capitalism. At any event Professor Hutt has produced an ingenious plan, even though it goes untried.

RUPERT B. VANCE

University of North Carolina

BOOK NOTES

Statistical Analysis for Students in Psychology and Education. ALLEN L. EDWARDS. New York: Rinehart and Company, Inc., 1946. pp. xviii + 360.

The author's declared intention is to reduce student resistance to statistical methods. Insofar

as a text can accomplish this task, the present one does. For beginning students of social statistics this text has one major deficiency explicit in the title, namely, the omission of any discussion of the collection, tabulation and graphic presentation of data. On the other hand, the

general simplicity of exposition is exemplary; the chapters on sampling theory are among the clearest in the literature. There are well selected problems, with answers provided, and all necessary tables.

Changes in Income Distribution During the Great Depression. HORST MENDERSHAUSEN. National Bureau of Economic Research. New York. 1946.

Family incomes in the very low and the very high income groups tend to suffer more than the income of those in the intermediate brackets during major depressions, according to this excellent study by a member of the faculty of Bennington College on the basis of data collected by the Financial Survey of Urban Housing by the Department of Commerce in 1934 as a Civil Works Administration project. Data as to family income in 1929 and in 1933 were collected from over 300,000 families in 33 medium and large-size cities, and for both 1929 and 1933 on more than 200,000 identical families.

Among the 33 cities, the general income level declined at rates ranging from 24 per cent for Richmond, Virginia, to 51 per cent for Racine, Wisconsin. The average decrease was 37 per cent.

United States Life Tables and Actuarial Tables, 1939-1941. By THOMAS N. E. GREVILLE. Sixteenth Census of the United States, 1940. United States Government Printing Office: Washington, D.C., 1946. pp. 153. \$1.25.

Contains life tables for each sex in the total population and each of the three racial groups: white, Negro, and other races. The twelve tables are based on the 1940 census of population and the deaths in the 3-year period 1939-1941 for the entire continental United States. Explanatory notes dealing with their construction and use accompany the tables. The actuarial tables, computed for whites only, by sex, are likewise explained in detail. The volume is a valuable addition to population literature because of the concise and lucid presentation of technical matters as well as for the tables themselves.

A Trial on Trial. MAXIMILIAN J. ST. GEORGE and LAWRENCE DENNIS. National Civil Rights Committee, 1946. 503 pp. \$5.00.

Defensive propaganda about the trial of the alleged seditionists at Washington in 1944—this time with the co-authorship of the leading fascist theoretician in the United States.

California Business Cycles. FRANK L. KIDNER. University of California Press. Berkeley and Los Angeles. 1946. xiv + 131. \$2.50.

Convincing statistical proof that California belongs to the Union. Indices of freight car loadings, building permits and other phenomena from 1919 to 1939 show that "the cyclical behavior of California is very similar to the cyclical behavior of the nation as a whole." (p. 106) Similarities are more marked during contraction periods than during periods of expansion. Good technical research report.

Shall I Get a Divorce—and How? JOHN H. MARIANO. Council on Marriage Relations, Inc. New York 1946. vi-141. \$2.

A handbook of practical advice by a marriage relations counsellor for people in marital difficulties. Deals largely with the legal requirements and procedures, jurisdictional conflicts and dangers, and with legal concepts such as adultery, cruelty, desertion, non-support, etc. Straight to the point, but hardly a substitute for a good lawyer.

The Bill of Social Rights. GEORGES GURVITCH. International Universities Press. New York. 1946. 152 pp. \$2.00.

A professor from the University of Strasbourg and editor of the *Journal of Legal and Political Sociology* while in this country during the war addressed himself to the problem of drafting a bill of rights to cover the needs of free men in the modern world. He states his conceptions under such headings as Right to Labor, Right of Labor, Right to Rest and Retirement, Right to Freedom of Labor Unions and Right to Strike; Social Rights of Consumers and Users; Social Duties and Social Rights Concerning Property; Social Rights of the Common Man. To verbalize liberal ideals abstractly may be one function of intellectual leadership, but most sociologists will probably be inclined to ask "So what?"

History of Legislation and Policy Formation of the Central Valley Project. MARY MONTGOMERY and MARION CLAWSON. U. S. Department of Agriculture, Bureau of Agricultural Economics. Berkeley, Calif. 1946. paper. litho. 276 pp.

A case study from public records of the geohawing that goes into policy formation in dealing with long-range development of water power resources in the American culture—this time, California. Deals with the period 1933-1945.

Social Correctives for Delinquency. Yearbook, National Probation Assn., 1945. MARJORIE BELL, ed. National Probation Association, New York. 1946. paper, 328 pp. No price indicated.

The usual useful compilation of articles on delinquency and crime, four of them oriented toward "War and the Offender." Ralph S. Banay calls for the establishment of an institute of criminal science. O. H. Close reports on "California Camps for Delinquents"; Edward E. Schwartz, on "A Community Experiment in the Measurement of Juvenile Delinquency" (the Children's Bureau's attempt to use the District of Columbia as a guinea pig in developing comprehensive delinquency statistics by having all children's agencies register all children under 18 referred because of alleged delinquent behavior.) In the District of Columbia the juvenile court got only 34 per cent of the 10,697 boys' cases and only 11 per cent of the 4,526 girls' cases. In the "Legal Digest" Phyllis Carlberg summarizes 166 items of legislative or judicial action in 41 states in 1945.

Men, Mind and Power. DAVID ABRAHAMSEN, M.D. Columbia University Press. New York. 1945. pp. vii-155. \$2.00.

A popularized explanation of how the Germans got that way by a medical man whose sociology is somewhat less up to date than his medicine. Thus we are told, "thousands of years ago the geography of Germany gave the German people a characteristic or a character trait—that of banding together" (p. 20). And again "Isn't it true that harsh sounds may call forth rude thoughts, or vice versa? And cannot rude thoughts produce rude or cruel deeds?" (p. 21). That has one or two points on Mark Twain's discussion of "The Awful German Language"! Psychiatric analysis by remote control is Dr. Abrahamson's specialty: he does it on Hitler, Quisling and Laval. If it's that easy, why do psychiatrists see their patients? An interesting example of the speculative method of explaining social phenomena.

Facing Your Social Situation. JAMES F. WALSH, S. J., Bruce Publishing Co., Milwaukee. 1945. pp. x-237. \$2.75.

An introduction to social psychology from the Catholic point of view. Five out of nine chapters discuss some form or aspect of situations in the usual terminology. Forming the Situation (Chapter V), for example, is a matter

of Suggestion, Imitation, Invention, Social Projection, and Identification. Ideological rather than experimental.

Our Atomic World. Los Alamos Scientists University of New Mexico Press. Albuquerque, N.M. 1946. 73 pp. ill. pamph. 50 cents. Foreword by Enrico Fermi.

The most compact and authoritative explanation of what atomic bombs are, what they can do, prospects for non-military uses of atomic power, and the technological basis of international control.

L'Antialcibiade, ou La Revolution des Faits. HENRI JANNE. Office de Publicité, S.C.L. Lebegue & Cie. Brussels. 1944. paper. 302 pages.

A French exhortation to democracy, "the only legitimate regime," to beware of demagogues and to make the economy of abundance work in one small world (une unité étroite) by institutional reforms on pain of "total disorder."

Detention and Prosecution of Children. Jail Detention and Criminal Prosecution of Children of Juvenile Court Age in Cook County (Ill.) 1938-1942. FRED GROSS, Dimmick D. Drake Fellowship Fund, Central Howard Association, Chicago. 1946. paper litho., 177 pages. \$1.50.

Despite state law dating from 1899 for treating juvenile offenders as state wards, Cook county and municipal officials in period indicated jailed 319 different boys and girls, almost half of them after indictments. No consistent policies of selection were followed. Hence, as in many other communities in other states, the problem remains whether the juvenile courts are to be permitted to perform the functions for which they were established.

The Roots of American Loyalty. By MERLE CURTI. New York: Columbia University Press, 1946. xi + 267 pp. \$3.00.

Problems of social unity have been of constant sociological interest, as shown by the literature on "solidarity," "integration," "group loyalty," "consciousness of kind," etc. Thus, not only historians but also sociologists will seek information in this work by a prominent historian which traces the development of the loyalty of Americans to their country. In the course of his analysis, Professor Curti deals incidentally with various matters which are of additional interest to sociologists. He presents

materials to illustrate attitudes towards immigrants and expressions of their loyalty; he discusses briefly the patriotism of racial minorities; and he surveys various means which tend to strengthen national loyalty. The author's selection of source material is likely to puzzle the sociological reader. "The main source for this study has been a body of material hitherto little exploited: the occasional sermon, the Fourth of July oration, and the academic address." How genuine an expression, how sensitive a measure of national loyalty as a social reality is a graduation speech, a patriotic sermon, an oration before an honor society, a Fourth of July address?

Science For Democracy. Edited with an Introduction by JEROME NATHANSON. 170 pp. New York: King's Crown Press. \$2.50.

The book reports the third annual Conference (May 1945) on The Scientific Spirit and Democratic Faith. Papers were read and discussions participated in by spokesmen for both the physical and the social sciences. In the words of the editor, the purpose of the Conference was to make "an attempt to grapple with the problem of making the scientific habit of mind an integral part of the democratic process." While the Conference was held some months before Hiroshima, the point was made at the meeting that science is becoming increasingly "the crucial key to power." The statement was also made that "power over science is to a usually unrecognized extent in the hands of private business." One of two symposiums was devoted to the question, Does Private Industry Threaten Freedom of Scientific Research? Those participating did not agree as to the answer. In closing, the chairman of this session remarked upon the need for further education. It was his opinion that colleges and universities should not engage in research for private concerns. The second symposium dealt with the topic, The Role of Science in the Determination of Democratic Policy. One of the speakers remarked, "Passion, prejudice, partisanship, unreason still sway men . . . precisely as if scientific method had never had heard of."

The Islands and Peoples of the South Seas and Their Cultures. By RAYMOND KENNEDY. A Jayne Memorial Lecture (University Extension Society) delivered at the American Philosophical Society, March 1944. Philadelphia, 1945. pp. 88 (paper bound; 1 map and 18 illustrations).

This popular, unannotated discussion opens with a brief consideration of the geography of the area, the racial composition of its population and the history of native migrations and contact with Europeans. The ensuing description of the culture of the islanders, though cursory, follows the broad outlines of accepted ethnographic classification. The original contribution of the lecture lies in its concluding section dealing with the aims and problems of colonial administration. Dr. Kennedy points out that the breakdown of native culture and the pressure on the native to assimilate Western ways raises the pertinent local problem of "education for what?"

The Absolute Weapon. Atomic Power and World Order. By BERNARD BRODIE, editor; Arnold Wolfers, Percy E. Corbett, William T. R. Fox. Introduction by Frederick S. Dunn. 165 pp. A Yale Institute of International Studies Publication. New York: Harcourt, Brace & Co. 1946. \$2.00.

This book, which is written in non-technical language, is a comprehensive consideration of atomic power and its relation to world politics. The various plans which have been advanced for the prevention of atomic war are critically examined. The authors agree that the best basis for hope that such a war may be prevented in our present world of conflicting values lies in the fear of retaliation. And they find little of realism in this hope. "The more essential condition of peace in an atom-splitting age, as before, is underlying acceptance of common values." The implication seems clear that these common values must be those of co-operation. "It is not only sovereign states but individual attitudes which must undergo transformation."

Government Against the People. By ASHER BRYNES. New York: Dodd, Mead and Company. 1946. 265 pp. \$3.00.

The author, a Fellow of the Guggenheim Foundation, 1938, 1939, and 1944, attempts to defend the interesting thesis that "that country will be most pacific in foreign relations which is least centralized at home." He traces the development of police forces in Russia, Great Britain, and the United States. As regards Russia, he concludes that, "What we can do to our lasting benefit as well as that of the Russians is to recognize that so long as the problems of defense and the fears of insecurity press upon this exposed land it will remain organized for war in peace as well as in war."

He furnishes the reader many detailed items of valuable historical information. The book, for the most part, is quite readable, with occasionally an especially good sentence, as when in reference to a certain Tsar's conscience, the author remarks, "He worried about it constantly and did little, as most of us do with our consciences." There is no bibliography.

Signs, Language and Behavior. By CHARLES MORRIS. New York: Prentice-Hall Inc., 1946. pp. vii + 365. \$5.00.

On a behavioristic foundation, Professor Morris erects a conceptual framework for the science of semiotics which is simple and rigorous. He avoids current terminological confusions by a continuous checking back to the behavior situation in which signs appear. His own terminology is systematically separated from and yet shown to clarify the issues raised by the various alternative formulations of other semioticians and related scientists.

For sociologists there are useful discussions of: language as an animal or a strictly human trait; George H. Mead's theory of the significant symbol, social roles and the self; the role of signs in thinking; culture as a sign configuration; and the social pathology of signs. The work as a whole is an example of closely reasoned, critical yet good-humored exposition which sociological theorists might do well to emulate.

The American Jewish Yearbook 5707 (1946-47) Vol. 48. Jewish Pub. Society of America: 1946. xii + 691 pp.

This is a valuable handy reference guide to Jewish material, containing as it does, a listing by states, of the directory of Jewish Welfare funds, Welfare Federations, Community Councils, and affiliated agencies. Some of the special articles are also of interest and value such as the article on the Jewish Population of Canada by Louis Rosenberg. Data presented under the heading "Statistics of Jews" has the same errors and limitations found in previous volumes.

Again with such material the need for accurate data for sociological research becomes evident. It may be hoped that in the U.S. particularly the question of religion will be included in the 1950 census as it has been included in Canadian censuses for sometime.

Inter-American Affairs 1945, An Annual Survey: No. 5. ARTHUR P. WHITAKER (ed.). Columbia University Press, 1946. 328 pp. \$3.75 (Cloth).

Sociologists will find three chapters of this survey particularly pertinent to their interests. A new contributor, William Ebenstein, presents a chapter, "Political and Social Thought in Latin America," full of sweeping but stimulating discussion of the influence both of the Catholic Church and Communism. "Labor and Social Welfare," by Otis Mullikan and Sarah Roberts, gives a résumé of conferences and social legislation somewhat along the lines of the reports in the *International Labor Review*. W. Rex Crawford's chapter on "Cultural Relations" gives an excellent account of the numerous developments in the programs for stimulating and facilitating cultural interchange in the hemisphere. Chapters on the economic, political and diplomatic phases of inter-American affairs round out this new survey volume.

Physical Examinations of Selective Service Registrants During Wartime: An Analysis of Reports for the Continental United States and Each State, April, 1942—December, 1943. Medical Statistics Bulletin No. 3. Prepared by Major C. H. Greve, V. H. McGill, and Colonel L. G. Rowntree. National Headquarters, Selective Service System; Washington, D.C. November 1, 1944.

This report deals with the results of the physical examinations of over 9,000,000 men as represented in a 20 per cent sample of the total. It analyzes rejection rates, causes for rejection the incidence of all recorded defects, and State and regional variations in rejection. An appendix contains approximately 70 pages of tabular data.